THE LANCET.

VOL. I.] LONDON, SATURDAY, FEBRUARY 17, 1838. [1837-38.

LECTURES
ON THE
THEORY AND PRACTICE OF MEDICINE;
NOW IN COURSE OF DELIVERY AT THE
THEATRE OF ANATOMY AND MEDICINE, WEBB-STREET, SOUTHWARK.
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&c., &c.

PHYSIOLOGY OF THE NERVOUS SYSTEM.—*Originality of the excito-motory doctrine. Conduct of the Council of the Royal Society. Detail of the functions of the excito-motory system. Excited closure of the eyelids. Physiology of deglutition. Action of the cardiac orifice of the stomach. Closure of the larynx, how produced. Physiology of respiration. Functions of the pneumogastric nerve. Closure of the sphincter ani. Action of the expulsors. Acts of generation. The tone of the muscular system. Seat of the passions.*

GENTLEMEN:—Before I commence the subject of the special acts of the true spinal or excito-motory system, I must very briefly advert to the point of the originality of my researches into this part of my subject. Let the hearer compare the works of Dr. Bostock, of Mr. Mayo, of Dr. Alison; of M. Magendie, and even of Prof. Müller (whose "Handbuch" may be considered as representing the physiology of the day more faithfully than any other, and is now partly translated into English by Dr. Baly), with the subsequent details, and then *let* HIM *judge.*

151. If I revert to this question more frequently than my hearers think necessary, let my excuse be, that my labours have been most unjustly opposed, that they have been opposed,—nay, even discarded, where they ought to have been welcomed. Will another generation believe that the paper which contained the views which form the basis of these lectures, was rejected by the *Royal Society*,—that, after being read, it was refused a place in the "Philophical Transactions;"—and that, when I offered to sacrifice five years of my life and professional avocations and advantages, to pursue this interesting subject with undivided attention, the Council even refused to look at my experiments, and that, as if to add *insult* to injury, they wrote upon my MS., in reference to the experiment of Redi, mentioned § 37, "*will they* (i. e. the *tortoise*), *live after they are made soup of?*"! * * * * I have been accused of having used too strong a term in speaking of this treatment. Let the hearer substitute any other which may appear to him more *appropriate! I* wrote as I felt, and still feel as I then felt and wrote. I consider that a great injustice has been done me; and I have the conviction that this theory—let them call it "hypothesis" who please—will long survive the little jealousies and rivalries of its opponents.

152. Every possible mode has been adopted to deprive me of the credit of having eliminated this theory out of the meagre materials to be found in the works of Whytt, Legallois, Mr. Mayo, &c. Anonymous critics, and commentators not anonymous, have united for this purpose. One of the latter is Mr. Owen (see vol. iv., p. 202, of the recent edition of the works of Mr. J. Hunter). I would ask Mr. Owen, whether any previous physiologist has shown that the *vis nervosa* of Haller, and the motive power in the experiments quoted, are *one and the same*, and that it has an important application to physiology, pathology, therapeutics, &c. I would ask Mr. Owen, whether it had been previously pointed out that the act of deglutition, that the action of the sphincters, &c., with *all* ingestion and egestion, *all* guarding of the orifices and outlets of the animal frame, were *spinal* functions, performed through special *excitor* and *motor* nerves? Mr. Owen's statement is very clear, but it is *ex post facto*. Mr. Owen's doubts are also clear, but it is admitted by me that the question is *sub judice*. *Jam satis.*

153. But I proceed to the *philosophy* of my subject. I proceed to treat of the acts of the excito-motory system. As the desig-

nation of this system implies, there is always the application of an appropriate stimulus, or cause of excitement or irritation; this is followed by the contraction of peculiar sets of muscles. It is clearly proved that the influence of the stimulus is carried along an excitor and incident nerve, to the medulla oblongata, or medulla spinalis, and that it is reflected thence along other reflex, or motor nerves. The incident excitor nerves, the medulla, and the reflex motor nerves, constitute the system. They remain, as I have already stated, after the centre of the cerebral system has been removed by experiment, or destroyed by disease. Their distribution takes place principally about the larynx and pharynx, in connection with the medulla oblongata; and about the sphincters, in connection with the lower part of the spinal marrow; and hence they especially guard the *orifices* and *exits* of the animal frame. Other parts of the system govern the acts of *ingestion*,—deglutition, and respiration;—and the acts of excretion,—of the fæces, urine, and semen. A third portion of the system gives general tone to the muscular system, and, consequently, to the limbs. I may observe that each of these actions may be represented to the *eye*, as will be seen in the subsequent sketches.

154. I now proceed to notice each of the functions of the excito-motory system particularly. Of these the figures at § 50 and 52 may be considered as the *types*.

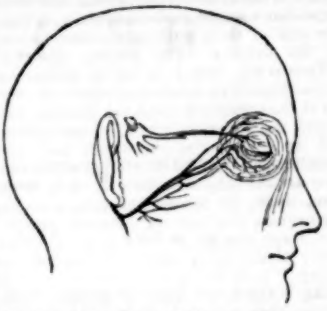
1. *The Excited Closure of the Eye-lids.*

155. This phenomenon has been described, § 118, in detailing the experiments on the horse. The general fact is, that, when the cerebral functions are annihilated in experiments on animals, and by disease in the human subject, as in the coma of apoplexy, of hydrocephalus, when *sensation* and *volition* are *extinct*, the touch of the finger or of a straw applied to the eyelid, induces its instant closure.

156. M. Magendie and Mr. Mayo fail entirely in any attempt to account for this interesting phenomenon, which may be taken as the *type* of the excito-motory actions. See § 150.

157. One of the most interesting circumstances of the true spinal or excito-motory system, is, that each of its acts can be expressed by mere lines, denoting the excitor and motor nerves involved in it, together with the central medulla. The subjoined sketch denotes the closure of the eyelids. The trifacial nerve, the medulla oblongata, the orbicular branch of the facial nerve, are the different parts of the reflex arc: or it may be expressed thus,—the excitor and motor nerves are two arcs, united by the medulla oblongata as their *key-stone*.

158. But the most remarkable circumstance connected with the closure of the eyelid, is its relation to the state of *sleep*. I have already stated, § 135 and 147, that the cerebral system alone undergoes this remarkable modification; that the true



spinal system does not sleep. There must be some remarkable reciprocity between the levator palpebræ and the orbicularis in reference to this phenomenon. When awake, the levator palpebræ is more powerful than the orbicularis; in sleep the orbicularis prevails. I believe the levator palpebræ, and, perhaps, the four *recti* of the eye, to be, of all the muscles of the animal frame, purely *cerebral* and *voluntary*, and unendowed with fibres from the excito-motory system. When awake, volition raises the eyelid. During sleep, the excito-motory property induces constant contraction of the orbicularis, as it does of the other sphincters. The eye is thus preserved from exposure during the night,—preserved from the state of inflammation with which it is attacked, when, from injury of the facial nerve, or failure of the excito-motory power, the tonic influence of the medulla is cut off, or diminished, and the action of the orbicularis is defective. See § 72. Similar observations apply to the action of the recti, compared with the trochlearis and abducens.

2. *The Act of Deglutition.*

159. The next part of the physiology of the true spinal marrow, and of the excito-motory system of nerves, relates to the act of deglutition. In treating of this subject, I must speak principally of the action of the pharynx, but also of that of the cardia.

160. 1.—*Action of Pharynx.*—If we press down the tongue with the handle of a spoon, and convey the instrument towards the root of the tongue and tonsils, an action of deglutition ensues. If, in a living animal, an incision be made in the side of the neck, and the finger be passed into the pharynx, it is immediately grasped forcibly. The same event occurs even after decapitation in a young animal. In this last case the action ceases either on dividing the nerves which intervene between the pharynx and the medulla, or by removing the medulla itself.

161. I have looked over the popular works of Dr. Bostock, M. Magendie, and Mr. Mayo, for any intimation of the real nature of the action of deglutition, and of its dependance on the medulla spinalis, in vain. The first of these authors is entirely silent on the subject. The second observes, "Thus is the second period of deglutition accomplished, by the effect of which the morsel of food passes through the pharynx, and enters the superior part of the œsophagus. All the co-operating phenomena take place simultaneously, and with great promptitude: they are not subject to the will; they, therefore, differ, in several respects, from the phenomena which belong to the first period." Mr. Mayo speaks of "the peculiar sensibility of the back part of the fauces" as being "excited," and of the act itself as being "instinctive and irresistible." Mr. Mayo adds, "if the action of deglutition be voluntarily performed several times in succession, and nothing but saliva swallowed, the parts become *fatigued*, and the operation cannot be immediately repeated." The real explanation of this last singular and interesting fact is this:—an excited act requires a stimulus or excitor; the saliva is that stimulus in the first and second acts of deglutition; but in a third, attempted promptly after the second, this stimulus is wanting; the act, consequently, fails for want of its excitor. The idea of "fatigue" is obviously fallacious.

162. In none of these authors is there the slightest allusion to the important and essential influence of the medulla oblongata in the act of deglutition.

163. 2.—*Action of the Cardia.*—The cardia opens to receive the food from the œsophagus, and closes to retain it in the stomach. It is paralysed on dividing the pneumogastric nerves. In a rabbit, in which this experiment is performed, the œsophagus is found replete with food, although it may not have eaten after the operation. The fact was first ascertained, I believe, by MM. Leuret and Lassaigne (p. 132). The pneumogastric is preeminently the internal excito-motory nerve.

164. The act of deglutition consists, then, 1, of an excited closure of the larynx; 2, of an excited action of the pharynx; 3, of an opening of the cardia. The nervous arcs which act in this apparently simple but really complicated phenomenon consist of excitor nerves *from* the larynx and pharynx, and of motor nerves *to* the larynx, pharynx, and cardia.

165. What shall we say of the apparently active dilatation of this last part,—*the sphincter of the stomach?* The act of forcing to expel the contents of the rectum produces a similar decided relaxation of the sphincter ani,—a fact perfectly familiar to surgeons who avail themselves of it, in order to effect the reduction of hæmorrhoidal tumours.

3. *Closure of the Larynx.*

166. The larynx closes accurately in every act of deglutition; on attempting to inspire carbonic acid; on the contact of a drop of water or a crumb of bread; in the act of vomiting, &c. What is the nature of this phenomenon?

167. If, in a living animal, or in an animal deprived of the cerebral lobes, or in the recently separated head of a young animal, the rima glottidis be touched with a feather, or probe, the glottis immediately closes firmly. This phenomenon ceases instantly, in the latter case, on the separating the larynx from its connections with the medulla oblongata, by a sharp instrument, within or without the spinal canal, or on destroying the medulla itself. It is, therefore, plainly dependant upon the medulla, and upon excitor nerves which proceed to, and motor nerves which proceed from, this part of the nervous system. It is a reflex, excito-motory act of the superior laryngeal, and the medulla oblongata.

168. M. Magendie has written expressly upon the action of the larynx; and though he concludes, from experiment and dissection, that the closure of the larynx depends upon the superior, and its opening upon the inferior laryngeals; and that it is essential that *all* be divided, in order that the larynx may remain open and immoveable, yet he is perfectly silent upon the essential agency of the medulla oblongata in all excited actions of the larynx. Mr. Mayo ascribes the closure of the larynx from the contact of carbonic acid, water, or mercury, to "the close consent between the sentient mucous surface of the larynx and its muscles."

169. I have thus demonstrated the essential connection of the eyelid, the pharynx, and the larynx, in their acts of closure, with nerves going to and from the medulla oblongata, and with the medulla oblongata itself. No previous physiologist has seen that these apparently simple acts are really very complicated, and that they are *spinal* acts. That deglutition is a *spinal* act, is a new view of the subject. I refer you *particularly* to the recent translation of Professor Müller's "Handbuch," by Dr. Baly, for proof of this assertion. I must next proceed to treat of another act of the same system, the most immediately essential to life of all, viz.:—

4. *The Respiration.*

170. That the respiration is a mixed function, and partly dependent on cerebral agency, or volition, is shown by the effect of profound attention, sleep, stupor, the sinking state, &c. During attention, or sleep, the respiration becomes irregular and audible; in stupor it is noisy and stertorous; in the dying state it is impaired and catching. In *all* these cases the respiratory movements are instantly improved by rousing the per-

son to sensibility and acts of volition. The partial dependence of respiration on the excito-motory power, is made manifest by the division of the pneumogastric nerves; the acts of respiration immediately become much more frequent; and, as M. Magendie expresses it, "the animal appears to devote particular attention to it."

171. From the multitude of opinions respecting the acts of inspiration, it will be sufficient to select three, to be noticed in this place.

172. First. Dr. Wilson Philip and Mr. Mayo consider the acts of inspiration as acts of volition, or of conscious effort.

173. Secondly. Dr. Bostock, Dr. W. Philip, and M. Brachet, consider the acts of inspiration as dependent upon the pneumogastric nerves, as nerves of sensation.

174. Thirdly. Legallois, Sir Charles Bell, M. Flourens, Professor Müller, &c., regard the acts of inspiration as dependent upon the medulla oblongata as their *primum mobile*.

175. It appears to me to be a sufficient reply to the opinion that respiration is an act of the will, or of conscious effort, that it is repeated when the cerebral lobes, the seats of volition and consciousness, are removed, and when all indubitably spontaneous acts have ceased.

176. And it appears to be a sufficient reply to the idea, that inspiration depends upon an uneasy sensation conveyed to the sensorium by the pneumogastric nerves, that it is repeated when these nerves are divided. Dr. Bostock and Mr. Mayo speak of this act, and of the agency of the pneumogastric nerves, as imperfectly understood. M. Brachet, speaking of the experiment in which the pneumogastric is divided, observes,—"In this case we must not attribute the continuation of respiration to the necessity which is felt of respiring, but to the *habit* which the cerebro-spinal nervous system has contracted, of causing the respiratory muscles to move."

177. I must now make some equally brief observations upon the opinion, that the medulla oblongata is the *primum mobile* of respiration. It is founded upon the facts, that the cerebrum may be removed from above downwards, and the spinal marrow from below upwards, without suspending the acts of inspiration, if the medulla oblongata, at the point of origin of the pneumogastric, be preserved entire. Yet, as M. Flourens observes, "the evident proof that it is neither solely, nor even precisely, because it is the origin of the eighth pair, that the medulla oblongata is the *primum mobile* of respiration is, that the two nerves may be cut, and respiration (although rendered constrained and laborious), would, nevertheless, subsist for a very long period." From these several experiments we should be apt to conclude, that neither the cerebrum nor the pneumogastric nerves, are necessary for the acts of inspiration, since these acts are renewed when either is removed. The truth, however, is, that, although the acts of inspiration continue without *either*, they will not continue without *both*. Each may be removed *singly*, but if *both* be removed, the inspirations cease, as in the experiment of dividing the medulla oblongata at the origin of the pneumogastric nerves, an experiment hitherto unexplained. In fact, inspiration may be a voluntary act, induced by the agency of the cerebrum, the pneumogastric nerves being divided; or it may be an excited act, excited through the agency of the pneumogastric nerves, the cerebrum being removed. If, in this latter circumstance, the pneumogastric nerves be divided, the acts of inspiration cease. In this last fact we have, then, the proof that the medulla oblongata is not the *primum mobile* of respiration, but that the pneumogastric nerve is that *primum mobile*, as an excitor of respiration, essential and necessary when the agency of volition, with its organ, is removed,—an important conclusion, by which many difficulties, and an interesting question, are at once solved.

178. The acts of inspiration, then, are acts of the excito-motory, or true spinal system. Ordinary inspiration is excited through the medium of the pneumogastric nerve, but regulated and controlled by the volition. During sleep, and in coma, the influence of volition is diminished, or annihilated, and the respiration becomes audible, or even stertorous. Respiration is, therefore, as stated of old, a *mixed* function, as, indeed, are many of the acts of the excito-motory system, excited through particular nerves, but regulated or modified by volition.

179. This remark leads me to observe, that the pneumogastric is not the only excitor of inspiration; inspiration is equally excited through the fifth and spinal nerves,—a fact proved by the familiar phenomena induced by dashing cold water upon the extremities of the former in the face, and by impressing the latter by a similar influence on descending into the cold bath. The first act of inspiration in the newly-born infant is probably excited through the medium of the fifth and spinal nerves conjointly, by the contact of the atmosphere, as the first acts of the expulsion of the fæces and urine are excited during the similar contact of the atmosphere with the extremities of the spinal nerves.

180. My friend, Dr. Heming, witnessed an interesting fact in proof of this opinion. The infant just born, and covered by the bed-clothes, did not breathe; Dr. Heming, after waiting a few seconds, proposed to himself to adopt some measure for this asphyxia, and lifted up the bed-clothes. The contact of the cool atmosphere instantly

excited an act of inspiration. This view of the subject is confirmed by some facts in pathology, to be detailed shortly, and by some experiments.

181. In the first place, if the pneumogastric nerve be laid bare on the neck of a donkey, and irritated by the forceps, an act of inspiration, followed by an act of deglutition, is actually and instantly excited.

182. In the second place, we have to consider what is the stimulus, or exciting cause of inspiration. I must adduce, in this place, the celebrated experiment of Hook, given in a very early volume of the "Transactions of the Royal Society," in which a stream of atmospheric air was driven through the trachea, the lungs, and incisions made through the pleura, in a living dog; the animal made no effort to inspire whilst this stream was continuous; but when it was interrupted, the efforts at inspiration were violent and convulsive. In other words, when the air respired was unmingled with the carbonic acid exhaled from the lungs, no act of inspiration was excited; but when it became charged with that gas, efforts at inspiration were immediately made. Is carbonic acid, then, in the air-cells of the lungs, and in contact with the fibrillæ of the pneumogastric nerves, the exciting cause of inspiration?

183. Other facts confirm this idea. Dr. Faraday particularly mentions the fact, that the respiration can be suspended longer after repeated deep inspirations, by which the air of the lungs is completely renewed, than in ordinary circumstances. Divers breathe less frequently in proportion as they breathe an atmosphere under augmented pressure, and consequently condensed,—the *proportion*, or bulk, of carbonic acid evolved being less speedily completed. Lastly, the number of respirations is gradually augmented, with gasping, as the proportion of carbonic acid in a given quantity of gas, in which an animal is confined, becomes augmented. It would be interesting to repeat this experiment, taking the precaution of keeping the proportion of oxygen gas the same. In a word, what pure carbonic acid is in contact with the rima glottidis, diluted carbonic acid is in the lungs—an exciting cause, acting through the medium of the excito-motory, or true spinal system.

184. This principle is corroborated by a circumstance, of which it alone affords an explanation. It has been observed, that there is always a certain proportion maintained between the number of pulsations of the heart and of respirations. This proportion obtains from the highest degree of activity in an animal, even to the most complete quiescence in hybernation. The evolution of carbonic acid is greater in proportion to the rapidity of the circulation; this carbonic acid is itself the exciting cause of inspiration; this act will, therefore, be repeated more or less frequently as the circulation, and with it the evolution of carbonic acid, is more or less rapid. I believe this law of the proportion between the circulation and the respiration, has not been explained before.

185. But the experimentum crucis consists in first carefully removing the cerebral lobes with the cerebellum, and then dividing the pneumogastric nerves within the cranium, or spinal canal, or in the course of the neck. Respiration subsists as a purely excito-motory act, performed through the agency of the pneumogastric nerves, when the cerebrum is removed, and it ceases immediately when these nerves are then divided.

186. It appears from these various facts, that the acts of inspiration are excited acts, and excited through the medium of several excitor nerves. These may be arranged thus:—

1. *The Trifacial.*
2. *The Pneumogastric.*
3. *The Spinal.*

But if these constitute the *excitor* nerves of inspiration, the medulla oblongata must be viewed as the organ which combines the various muscles into a system; and the various nerves comprised in the respiratory system of Sir Charles Bell, are the true *motory* nerves of respiration. I may take this opportunity of remarking that, beautiful as it is, Sir Charles Bell's system of inspiratory nerves is defective from *two* circumstances; first, it is confined to the motor nerves, of the respiratory system; secondly, it is confined to respiration, when it ought to be extended to the whole of that system, which I have ventured to denominate excito-motory, and which relates, not to respiration alone, but to all the acts of ingestion and egestion.

187. I might further adduce, if it were necessary, experiments of M. Dupuy, M. Cruveilhier, and of Sir Astley Cooper, in favour of this view of respiration. M. Dupuy, forty hours after having divided the pneumogastric nerve in the horse, found irritation of the divided end of the upper portion to induce great labour in respiration. M. Cruveilhier, after destroying the function of the cerebrum, found respiration to cease instantly on dividing the pneumogastric near its origin. Sir A. Cooper observed that the respiration became gradually slower on dividing the pneumogastric, and to cease instantly on compressing the carotid and vertebral arteries simultaneously, whilst the action of the diaphragm ceased on dividing the phrenic nerves. These facts prove the respective influence of the incident, excitor, nerve—the pneumogastric; of the centre of the system—the medulla oblongata; and of the reflex moto nerve—the diaphragmatic.

188. In the separated head of the turtle

an act of respiration is excited either by irritating the nostril, the larnyx, or the cut surface of the spinal marrow. The action is alike—equally excited—in all.

189. The pneumogastric pursues a singular course in fishes. After giving branches to the bronchiæ, it proceeds along each side of the body to the tail. Mr. Owen calls these the "enigmatical nerves," and an enigma will they be, until the excito-motory system is *understood* by those who pretend to criticise it.

5. Closure of the Sphincter Ani.

190. The sphincter ani and cervix vesicæ present precisely similar phenomena with the larynx and pharynx. In a horse, rendered insensible by a blow of the poleaxe, the sphincter was forcibly contracted, and the tail raised, on stimulating the border of the anus; these phenomena ceased either on separating the sphincter from its connection with the spinal marrow, or on destroying the spinal marrow itself: the excitory and reflex action of the medulla, and its influence on the sphincter are therefore obvious. Dr. Alison describes the action of the sphincters in one work (*Outlines*, 1833, p. 7) as dependant upon what he designates tonicity, a property of the muscular fibre; in another (*British and Foreign Med. Review*, vol. iii. p. 33), as belonging to the class of sympathetic actions. It is plain, from this discrepancy of opinion in the same author, that there was nothing definitive in our knowledge on this subject, until I proved that the action of the sphincters, like that of the orifices, depends on the excito-motory property, acting through incident, excitor nerves, the spinal marrow, and reflex, motor nerves.

6. Action of the Expulsors.

191. If, in a turtle, after the removal of the tail and the posterior extremities, with the rectum, and of course with a portion of the spinal marrow, water be forced into the intestine, by means of Read's syringe, both the cloaca and the bladder are fully distended before any part of the fluid escapes through the sphincter, which it then does on the use of much force only, and by jerks. If, when the cloaca is distended, the integuments over it are stimulated, the water is propelled to a considerable distance. The event is very different on withdrawing the spinal marrow: the sphincter being now relaxed, the water flows through it at once, in an easy continuous stream, with the application of little force, and without inducing any distention of the cloaca. The action of the sphincter, and of the expulsors of the cloaca, in the turtle, and its dependance on the spinal marrow, are distinctly proved by this interesting experiment.

7. Acts of Generation.

192. Erection of the penis may be an excito-motory act. It was induced in the patients with injury or disease of the spinal marrow, whose cases have been already noticed, each time the catheter was introduced into the urethra,—the patients themselves being unconscious either of the contact of the instrument, or of its effect.

193. That the emission of semen depends upon the same excito-motory function of the spinal marrow, is equally obvious. In ordinary circumstances it is excited through the influence of the nerve termed the dorsalis penis. It has been excited, in experiments made by M. Segalas, by acting immediately on the spinal marrow.—"If, in a male guinea-pig, of which the brain has been laid bare, says this physiologist, a stilette be plunged into the cerebellum, *so as to arrive at the upper part of the spinal marrow, erection* is produced; if the stilette be then introduced into the vertebral column, as far as the lumbar region, *ejaculation* takes place, whilst the bladder, if it were full, still preserves its contents. The same phenomena are observed in decapitated guinea-pigs, when a similar operation is performed with a stilette from above to below in the spinal marrow." This fact is confirmed by a remark of the late Mr. Earle,—that priapism exists in injury of the spinal marrow, only when that injury is in the *neck*.

194. I may revert to the case of the act of deglutition; deglutition cannot be performed several times in rapid succession, for want of the presence and stimulus of the saliva. In the same manner the act of ejaculation requires the *local* stimulus of semen.

195. That the grasp of the Fallopian tubes is excited on the same principle is extremely probable; and that the action of the uterus belongs to the same excito-motory system seems to be proved by the occurrence of expulsion of the fœtus after the cessation of respiration. Such an event is noticed by Professor Müller, and a recent case in point is given by Mr. Ingleby, of Birmingham.

196. In this manner we observe that while all the orifices of the animal frame, with the acts of ingestion, are under the influence of the reflex excito-motory function of the spinal marrow, the sphincters and expulsors also act as parts of the same system.

8. The Tone of the Muscular System.

197. Of this function the type has been given already. There is another phenomenon belonging to this system which demands our attention,—the tone of the muscular fibre throughout the animal frame. Two rabbits were taken: from one the head was removed; from the other, also, the head was removed, and the spinal marrow cautiously destroyed by a sharp instrument; the limbs of the former retained a certain degree of firmness and elasticity; those of the second

were perfectly lax. The difference was most obvious. On the following day the limbs of both were found equally rigid from the contraction of the muscular fibre from its irritability.

198. The dependance of the muscles on the influence of the spinal marrow, for tone, is extremely obvious in the turtle.

199. The limbs and tail of a decapitated turtle possessed a certain degree of firmness or tone, recoiled on being drawn from their position, and moved with energy on the application of a stimulus. On withdrawing the spinal marrow gently out of its canal, all these phenomena ceased. The limbs were no longer obedient to stimuli, and became perfectly flaccid, having lost all their resilience. The sphincter lost its circular form, and its contracted state; became lax, flaccid, and shapeless. The tail was flaccid, and unmoved on the application of stimuli.

200. It is perfectly obvious, from this experiment, that the tone of the muscular system, and the action of the limbs on the application of stimuli to the skin, are modifications of the same function. Both coexist, or cease, with the spinal marrow.

9. *The Seat of the Passions.*

201. There is good reason to conclude, as I have already stated, that the cerebrum is the seat of the ψυχη, and of the *intellectual faculties*. There is equally good reason to believe that the medulla oblongata is the seat or nervous organ of the manifestation of the *appetites* and *passions*.

202. In the idiot, in whom the cerebral lobes are struck with such atrophy and defective development as to annihilate every vestige of intellect, the appetites are frequently not only unimpaired, but unnaturally strong: the appetite for food, sexual excitement, anger, and terror, are manifested in their turns in a remarkable degree.

203. The arm which is totally paralysed to volition or voluntary motion, in hemiplegia, is strongly agitated by surprise and other emotions. The seat of these emotions is, therefore, placed *lower* down in the nervous system than the seat of volition, and of the disease; whilst that of passion is manifested in the most distinct manner. The case is very different in paraplegia: here, the influence of the passions or emotion, as well as of volition, is totally cut off by the disease; that disease is, therefore, situated *below* the seats of volition and of the passions.

204. Have we not, in these cases, evidence of the actual seat of the passions? Is not this seat obviously placed *below* the seat of the disease in *hemiplegia*, and *above* that of the disease in *paraplegia*? And, if so, is not this seat the medulla oblongata?—the established centre and combiner of the acts of deglutition, and of respiration,—acts so important in reference to the appetite for food, and for air, the latter of which is affected in so extraordinary a manner in the exercise of the remaining one, and, indeed, in all the emotions and passions.

205. I mention this subject thus briefly only to propose it as a subject for future inquiry, in the course of these researches, and to render this sketch of the nervous system a little less incomplete. No one can see more clearly than myself how much remains for observation to accomplish.

206. It is interesting to remark that the passions affect precisely those organs of ingestion or egestion which are known to be particularly under the influence of the spinal system: grief induces a painful sense of choking; fear relaxes the sphincters; all the passions affect the respiration; a disgusting object induces sickness.

207. Before I conclude, I must inform you that there is a paper in the last number of the "Edinburgh Medical Journal" (for January, 1838), by Dr. J. Read, of which it is impossible for me to speak too highly, to which, however, a note is appended (p. 156), of which I cannot speak in similar terms of praise. Dr. Read's experiments confirm my own in every respect, and, I think, add nothing to what I had done. But they are invaluable additions to our store, and bear the stamp of great accuracy. It is important to bear in mind, in similar experiments, that the *trunks* of nerves by no means always display the properties possessed by them, in the points of their ultimate distribution. This last fact is proved by an experiment. Let a frog be decapitated, and let the skin along the spine be divided longitudinally, and raised with the forceps; many minute cutaneous nerves will be seen; now, I have not been able to excite motions by irritating these nerves, although nothing is so easy as to effect this by irritating the skin in which they are distributed.

TURPENTINE IN SCIATICA.—M. Ducros has frequently found sciatica, which had resisted the ordinary means, yield to enemata, containing a large dose of the essential oil of turpentine. He has related several successful cases. In one instance, the pain yielded to one enema, containing an ounce of the oil of turpentine, in another, six lavements were administered in three days, when the neuralgia gave way; in a third, the quantity was raised to two and a half ounces in each lavement; in a fourth, the lavements were continued for a fortnight.

LECTURES
ON
MATERIA MEDICA AND THERAPEUTICS;

NOW IN COURSE OF DELIVERY
AT THE
WINDMILL-STREET SCHOOL OF MEDICINE.
BY
GEORGE G. SIGMOND, M.D.

SARZA SARSAPARILLA.

SARSAPARILLA.—*Analysis of the sarsaparilla-Report of MM. Souberain and Cuillerier. Characters of the genuine drug. Preparations in use. Decoction of sarsaparilla. The cold Liquor Sarsaparillæ. Inutility of the syrup. The compound decoction. Lisbon diet-drink. Effects of sarsaparilla in the cure of disease. Causes of its occasional failure. Indications for its employment in the venereal and other diseases. Recipe for the German diet-drink.*

GENTLEMEN:—Galileo Palotta, an Italian physician, in the year 1824, made a careful analysis of sarza, and gave to its active principle the name of parigline; about the same time another physician of the same country announced, that he had discovered a new principle, and this he called smilacine; neither the labours of Palotta nor of Folchi attracted the attention they deserved, either in this country, or in France, although M. Miguel, the editor of the "Gazette de Sante," announced the discovery in Paris of parigline; and THE LANCET, in the year 1825, gave the process by which the analysis was performed, and the experiments which the discoverer made upon himself with the new medicine. In 1831 Thebœuf called the attention of the French chemists to another substance which he had obtained, and which he called salseparine; in 1833 the indefatigable Batka, of Prague, announced the existence of an acid which he named the acid parillinique. In a letter addressed to Pelletier, which is to be found, or at least extracts from it, in the January number of the "Journal de Pharmacie" for 1831, is given the characteristic properties of this acid, and the analysis of sarsaparilla, which be believed to be formed of crystallisable matter, which is the parillinic acid, of a crystallisable colouring matter, essential oil, gum, bassorine, amidon, albumen, extractiform matter, gluten and glaiadine, fibrous and cellular tissue, pectic acid, acetic acid, salts, chloruret of lime, potass, magnesia, carbonate of lime, oxide of iron, and alumen. In the same year Poggiale published a very interesting paper, with a series of very useful experiments, and he arrives at the conclusion, after having procured the four substances, that Palotta was the first who discovered the active principle of sarsaparilla; that smilacine, salseparine, and the parillinic acid, are the same as parigline, obtained by different processes; that the properties of them all are the same, and that analysis proves that their elementary composition is the same.

At the sitting of the *Academie Royale de Medecine*, in the same year, a report drawn up by Messrs. Soubeiran and Cuillerier, was read; the first of these eminent men devoted his attention to the chemical inquiries as to the real principle; the latter undertook the consideration of its medicinal effects. It was then acknowledged that these four principles were identical; that a white, inodorous, slightly nauseous substance, unalterable on exposure to atmospheric air, crystallisable, was obtained; the results of the trials made at the *Hôpital des Veneriens* were read. Under the auspices of Soubeiran nine individuals were treated by it, but only one was cured. The therapeutic agency was, however, acknowledged to have been but insufficiently tried, and all opinion was suspended as to its efficacy until some further experience had been gained. Since that period there has been but little activity on the subject; some few indecisive experiments have been tried, which certainly prove that there is some particular principle which produces a sedative, and likewise a diaphoretic property; that it has power in chronic rheumatism, and in many of the diseases in which sarsaparilla has been successfully employed; but as there is an enormous expense attendant upon the steps of all the processes that have been pursued, there promises to be little probability of any very great advantage springing from the prosecution of the labours of those who have so decidedly shown to us the perfection to which the science of vegetable chemistry has in a few years attained.

The inactivity of sarsaparilla is very generally to be attributed to the carelessness which the greater number of druggists have shown as to the selection of the drug, and to the inattention paid to the processes by which its inherent virtues are to be extracted: a great deal of that which is to be found in the ordinary shops is quite inert from age, and from heedlessness in preserving it. You should, when you intend to prescribe it, be careful, if possible, to see and examine for yourselves what you are about to recommend to your patient; you must remember that it is a medicine which is not merely to be taken once, and if it fail in its operation that you can quickly remedy it by ordering something else to be substituted for it, or a dose to be repeated, for to be of any service in disease sarsaparilla requires to be persevered in for a considerable length of time; it is a remedy adapted to chronic complaints, and its effects are

not speedily visible. That which is in rolls is to be preferred to that which has been split, for then too large a surface has been exposed to the action of atmospheric air, and to many causes which must operate upon delicate vegetable matter. Chew a little, you will perceive a peculiar taste, somewhat bitter, or acrimonious; it will a little, if it be good, excite a secretion of saliva; it will also have a mucilaginous taste.

The root has occasionally been recommended in powder, in the dose of a scruple to a drachm, three or four times a day; but I believe very little advantage has been gained by it in this form. Sometimes as much as an ounce has been given in the course of the day; the only reason I have ever known assigned for prescribing the powder in preference to the preparations, has been, that a larger quantity might thus be used, as it could, without materially influencing the food, be taken with the meals.

The roots yield up all their virtues to water; and a sufficient maceration, either in cold or hot water, will, beyond the slighest doubt, be found infinitely superior to any other mode of operating on the vegetable matter they contain. The liquor thus obtained should very quickly be employed, for upon long keeping an alteration of the principles of which it is composed occurs. Notwithstanding so much has been written on the inutility of long maceration, and of long boiling these roots, still I am afraid there are very few who are aware of the inert medicine that results. I must express my dissent from the formula of the London Pharmacopœia, for I am pursuaded that the boiling down to two pints is injudicious. I am aware that the original authority was both that of the Spaniards, and that of a very practical man, and that the paper of Fordyce, published in the first volume of the "Medical Observations and Inquiries," has always been highly estimated, and certainly was the means of generally calling the attention of the profession to the preparations of sarsaparilla.

The formula, as it now stands, is:—Take of sarsaparilla, sliced, five ounces; of boiling distilled water, four pints; macerate for four hours in a vessel lightly covered near the fire, then take out and bruise the sarsaparilla; when bruised return it to the liquor, and again macerate in the same manner for two hours; afterwards boil down to two pints, and strain. The long boiling most undoubtedly very materially affects this preparation, and I very much prefer pouring boiling water upon the bruised sarza, and allowing four hours to elapse without any disturbance of the vessels. Dr. Hancock has very judiciously written upon this subject. Mr. Battley's affusion of cold water and maceration afford a most important remedy, to which he has given the name of liquor sarsaparillæ.

The old South American method of preparing it was by macerating an ounce of the root in about four pints of water for twenty-four hours, and boiling it away to one half; of this the dose was half a pint twice a day, four hours before the meals, and it was given in bed, covering the patient with clothes, so that a great sudorific effect might be obtained.

Monardes, who is the great authority from whom we derive all our knowledge of the manner in which the Spaniards found that the native Indians employed their remedies, informs us that the Indians take nothing but the decoction and the mucilage, for the first three days, and afterwards only almonds and raisins during thirty days course of it. Fordyce's formula is given, in consequence of his observing that "the surgeons failed in its use, because they all erred in macerating it so long in the water, before they boiled it, that it was spoilt for use for the next day." He says, "thus it fell into disrepute, and by degrees was almost forgotten, insomuch that of late some of our greatest masters in medicine have asserted that its decoction is no better than barley-water: he says he gives it in the following manner:—To three ounces of the sarsaparilla root, as fresh as it can be procured, not spoiled with age, worms, sea water, or moisture, add three quarts of river water, and bring it to boil immediately in an open vessel,—I used always a copper one, and let it boil away to two pints of the strained liquor, that is to about two pounds avoirdupois. I sometimes add to it a little liquorice root to make it more palatable. Every other day it must be made fresh, and what is not used the day it is boiled must be kept in a cold cellar." This last observation I would advise in almost every preparation of sarsaparilla to be strictly attended to, and I am persuaded you will then have occasion to be fully satisfied with its efficacy.

If the decoction be objected to by me, you must, of course, feel that I am still more strongly opposed to forming an extract from it in the manner directed by the London Pharmacopœia, and which has been condemned by the greater number of practical followers of pharmacy. It is thus made:—Take of extract of sarza two pounds and a half, boiling distilled water two gallons; macerate for twenty-four hours, then boil down to a gallon, and strain the liquor while yet hot; lastly, evaporate to a proper consistence.

Both Braude and Phillips observe that there is much difference of opinion respecting the activity of this extract, amongst those who admit the efficacy of other forms of sarza. It is certainly the worst preparation of that remedy, as it is usually met with, for it is easily decomposed by heat, and always suffers more or less during the protracted evaporation that is required.

There is a syrup of sarsaparilla which is sold by some individuals, and is a favourite remedy; its mode of preparation has been kept secret, so that I can give you no information on the point. Dr. Hancock has in his very valuable paper on the various remedies in chronic disorders, these sensible remarks,—"Certain sages of our profession have assigned to the useless extract of the London Pharmacopœia, and that not less useless syrup of sarsaparilla which is prepared from it, the best offices, when, in prescribing the decoction, they say, thicken it with extract, and sweeten it with syrup. We have seen these boasted extracts and syrups used in great quantity, and at great cost, but in vain, when afterwards a quart of the strong infusion has removed all the violence of the symptoms." The syrup of sarsaparilla of our College is declared to be inferior to the one to which I allude, which is prepared by Mr. Hudson; this has been tried at *St. George's Hospital*, and has been preferred; but the difference in the price shows there must be great expense in the process, for whilst the syrup of the Hall cost seven shillings and sixpence a pint, the other extracted from the hospital funds no less than eighteen shillings and sixpence; certainly a few drops of a good medicine are preferable to drachms of bad, and in the end are much cheaper. The formula of the Pharmacopœia directs fifteen ounces of sarza, sliced, boiling water a gallon, sugar fifteen ounces; macerate the sarsaparilla for twenty-four hours; then boil down to four pints, and strain the liquor while hot; afterwards add the sugar and evaporate to a proper consistence.

The last preparation to which I shall have occasion to call your attention, previous to the more interesting inquiries into the medicinal virtues of this drug, is one which, like many others, has had its advocates and its opponents, and which, if properly used, deserves rather to be commended than either abused or laughed at,—a fate that sarsaparilla has particularly met with. Dr. Elliotson, in one of his clinical lectures, has said, "it is very well known that while surgeons have great faith in sarsaparilla, physicians have very little faith; this is a strange anomaly, surgeons laugh at doctors and physicians, except when they are ill, and in return are laughed at by doctors for their extraordinary faith in sarsaparilla. I am satisfied of this (he continues) that sarsaparilla is used both by physicians and surgeons in a great number of cases where it does no good, and where it is not calculated to do any good; at the same time I am not prepared to say sarsaparilla has no virtue at all; it would be presumption in me to say so after all that has been said in its favour by various individuals." The decoctum sarzæ compositum is a substitute for a medicine which, at one period, was highly prized throughout Europe, and which, in forms which differed slightly from each other, was known under the name of the Lisbon diet-drink. It is thus formed:—Take of decoction of sarsaparilla, boiling, four pints, sassafras, sliced, guaiacum wood shavings, liquorice, bruised, each ten drachms; mezereon, three drachms; boil for a quarter of an hour, and strain; and of this medicine the larger the quantity that can be taken without any material inconvenience to the stomach, in the course of the day, the more speedy and marked is its efficacy; the quantity that has occasionally been drunk has been enormous, and as may be expected, from the want of proper precaution as to its proper preparation, without producing those beneficial effects which the sanguine admirers of the drug had prognosticated. In the older pharmacopœias of the different colleges of the Continent, you will find that some of the preparations of antimony were added to the Lisbon diet-drink. The addition of the guaiacum was made in consequence of the very high reputation which it had obtained throughout Italy and Spain, in the cure of syphilis; Vesalius and Fallopius both, in most instances, gave it the preference to sarsaparilla; the latter calls it more powerful, but says that the sarza has many noble qualities which, in some instances, render it superior. He prescribed equal quantities of the two decoctions. In the *Hospital for Incurables*, at Florence, it was said that the two drugs were employed in equal proportions, with the greatest success; but the celebrated Targioni Tozzetti affirms such could not have been the case, for the annual consumption, within the walls, of sarsaparilla, was six hundred and fifty pounds, whilst that of the guaiacum wood was only six. Some persons have ascribed all the energy of the Lisbon diet-drink to the mezereon; upon what principle I am unable to state, for scarcely any one who has tried this medicine, places much reliance upon it. Dr. Alexander Russel, in 1766, made some efforts to introduce it into practice as a substitute for the Lisbon diet-drink, which he had been induced to give, with the happiest effects, in removing syphilitic nodes and nocturnal pains; and, as he was satisfied with the efficacy of the decoction in the form originally recommended, he resolved upon a trial of the mezereon root in one more simple, and accordingly he prescribed a decoction of it, and he narrates some cases of its success. He says that Dr. Fothergill had also thus employed it; but a perusal of the cases narrated by Dr. Russel, will satisfy any reader that not the slightest practical deduction of the value of the mezereon employed alone can be drawn, for in every instance almost the sarza was conjoined to it, or a preparation of mercury was given. If you will look into the third

volume of "Medical Observations and Inquiries," you will see the account of the experiments made with the mezereon, and I think you will agree with me they are anything but satisfactory.

The little that I know of mezereon from my own practice, and I must confess I have tried it very seldom alone, and then in the form of the Edinburgh decoction, leads me to acquiesce in the opinion of Mr. Pearson, who says, "It has not the power of curing the venereal disease in any one form; except in an instance or two of lepra, in which the decoction conferred a temporary benefit, I have very seldom found it possessed of medicinal virtue either in syphilis or in the sequelæ of that disease, in scrofula, or in cutaneous affections."

I shall now speak to you of the effects of sarsaparilla in the cure of disease, regretting with Mr. Mackenzie, who has a paper upon the subject of the decoctions of the Pharmacopœia, in one of the late volumes of THE LANCET, "that the destructive methods of forming extracts should still be continued by the College, and that the old *cookery* plan, with respect to decoctions, should be perpetuated." When I call sarsaparilla a very valuable alterative, you will naturally inquire what I mean by such a term, one that is found very commonly in your older medical works, but which is used with great hesitation by the writers of the present day, who are more explicit in their definitions, and who very properly require that some well-applied name should be given to the different modes of action upon the human economy of all the remedies which are made use of. This term sprung up amongst the humoral pathologists who entertained the belief that the diseases of the human body arose from some disordered state of the fluids, and that all disorders were mere efforts of nature to expel the peccant humour; they considered the blood as the principal source of mischief, and they employed such medicines as might alter its state; hence they talked of purifying and sweetening the blood; and, fifty years ago, we had in our English Dispensary a formula called, decoctum edulcorans, or the sweetening decoction; it was made of sarsaparilla and China roots, and the observation is, "that it ought to be made the common drink in all ill habits, as dropsies, and venereal infections, and such as proceed from too much moisture, or from fluids too sharp and saline."

A vast number of chronic diseases which may soon wear a more acute form, depend upon some error of assimilation, by which the food is converted into a blood which does not give to the organs by which the actions of life are carried on, a due proportion of vitality, or which stimulates them in too high a degree; the great aim, then, of the organs destined for excretion is to take up from the blood that which is circulating to the injury of the more important tissues, and which, owing to some disordered state of the functions of the alimentary system, has not been carried off by the excrementitious canal. Thus the skin and the kidneys are called into unusual action; eruptions of the surface of the body, deposits in the urinary fluid, are the most ordinary consequences of this state; but where the functions are not duly performed, or where these organs have not the power of taking on an increased action, various disordered states arise, such as depositions in the glands, causing scrofulous swellings, various kinds of diseases of the skin, inflammations of joints, or of membranes, attended with effusions. The great skill which the medical man exhibits, is in discriminating what is the original cause of the disease, and its precise seat; he must always bear in mind that what he hears complained of, what his eye witnesses, is but the effect. He who sees in a child a swelled upper lip, or a soreness of the edges of the eyelids, does not order a local application to the one or the other, he knows that there is disorder in the primæ viæ; he suspects worms; if he does not alter this state of the digestive canal, he knows what the consequence will most probably be, that the digestive organs will soon partake of this disorder; that there will soon be an indurated state of the glands about the lower jaw; that there will be a prominent belly; that there will be a craving appetite, and soon an emaciation of the body; and that diseases will be developed which will gradually waste the system.

One of the errors into which the nosologists have led the profession, with all the great good they have done, has been to make the giving a name to disease from the combinations of symptoms, one of the proofs of skill; many an individual is perfectly satisfied with himself when he pronounces what he sees to be jaundice, or scrofula, or dropsy in the chest, quite forgetful that the true sagacity is exhibited in discovering what has produced these signs of disordered function. I believe there may yet be individuals who, the very moment they think they know what they are to nickname the complaint, feel convinced they are justified in having recourse to medicines which act upon particular organs, and they set to, pell mell, to pour in a most incongruous mass of remedies; all the articles in the materia medica are successively tried; and the practitioner sits down fully persuaded he has done everything that art could suggest, and, if he has poured in the last new fashionable drug, according to the dictum of the last recognised scientific physician, he has kept pace with the learning and the science of the age, and has done his duty to himself and to his patient; but you, gentlemen, are aware that to investigate narrowly the causes

of each symptom is your duty; you who know the healthy functions of every organ and tissue of the body, can trace each disordered state; you can point how one deviation from a natural state produces another; you can follow up the succession of phenomena, and you always find that trifling errors, from the slightest causes, may be productive of mischief to the whole of the beautiful mechanism, and you know that, by appropriate remedies, you can obviate causes.

Sarsaparilla was supposed "to correct the acrimony of the fluids," and, sometimes, "to wash out the lymphatic system," and hence to be a very valuable alterative; you will, however, find that it corrects the errors of digestion which are symptomatic of disordered states of the body, and which have been induced, not by anything which produces a faulty decomposition of food, but by that which has excited one or more of the secreting glands, belonging to digestion, into unwonted action; hence its great and important use in diseases produced by mercury, which show their primary effect upon the liver, their secondary effect upon the glandular system, upon the skin, or upon the immediate coverings of the bones. There is something in the mucilaginous principle of many plants that bears a strong resemblance to the salivary and to the pancreatic fluids; this abounds in sarsaparilla, and may, therefore, be a substitute for these secretions, which are so much influenced by mercurial treatment, and which may not possess that power of sufficiently diluting the bile when it is much increased. The circulation soon becomes slightly increased; the pulse is quicker and fuller under the influence of this remedy, and the kidneys having received from the blood more of that which is to be excreted, the urine soon becomes more high coloured, and the capillary vessels being fuller, the animal heat is increased, until, at length, the skin is called into increased action; at first a warm and equable perspiration is produced, which may be augmented until a sudorific effect attends upon it. Thus an alteration gradually takes place in all the secretions, which is not the result of two or three administrations of the remedy, but is the consequence of a slow and well kept up action upon the primæ viæ, in the first instance, and in the second, upon the circulating fluid.

It is not a remedy which produces tone on the stomach by any astringency, nor is it capable of retarding fermentation, nor does it seem to correct the vitiated secretion of the gastric juice, for no immediate effect is perceptible upon the stomach, but the different fluids subservient to the organs of digestion, that are poured into the intestinal canal, are improved, and even where the liver has been enlarged, where mercurial preparations have been in frequent use, the remedy has been most serviceable. Under its use those who have been rendered feeble and miserable by mercury, have speedily acquired an improvement in health, in appearance, and in spirits; and in most cases in which that mineral becomes necessary, you will observe that sarsaparilla assists the constitution to bear up against its destructive influence, and enables the invalid to go a long course of mercuey.

Many of those who have underrated sarsaparilla, have alluded to its not fulfilling the expectations which, they say, were excited by its admirers as an antisyphilitic. Now, the earlier introducers of this root never pretended that the disease was to be cured by it, without the aid of mercury, at least in Europe. It is true that Franciscus Ximenes, in his notes to Margraave's "Natural History of the Brazils," says, that there is nothing better where the lues is not inveterate, than the decoction, with a proper diet, and that in a few days health is restored; but that is not medical authority. Vidius gives it the third rank. Trincavellius only says, that it has greater power than a decoction of guaiacum. Fallopius, Massarius, Rudius, Nicolaus Massa, and Alexander Petronius, only speak of it in reference to that wood. Fordyce and Donald Monro, to whom we must look in this country, as the original introducers, thought that the sarsaparilla root was only to be depended upon in venereal cases, where mercury had failed, or had preceded the use of the decoction, or when it was combined with it, and that it was not to be trusted alone.

Dr. Donald Monro, who was the person who made us acquainted with the secret of preparing the Lisbon diet-drink, as ordered by the physicians at Lisbon, expressly states that it is efficacious in removing those venereal complaints that frequently remain after salivation, and the free use of mercury; and he says it is probable that the practitioners at Lisbon who prepare this drink sometimes add privately the solution of corrosive sublimate, or some other preparation of mercury.

The words of Cullen are constantly quoted, but it is always to be borne in mind, that whatever is the value of his nosological arrangement, that he can never be considered as a guide in the materia medica. Although I am fully aware of his claim to great sagacity in searching out the indications of cure, I cannot admire what his biographer has called "the enlightened scepticism with which he examined the chaos of materia medica;" I am one of those "who perceive traces of senility" in his treatise on that subject. I do not believe one syllable he ever wrote on the articles of the materia medica does any credit to his great talents; but the doctrines of general therapeutics which he laid down, are of the utmost importance; and, indeed, almost all we possess

in the English language upon that neglected branch of science, is copied from him. He says, with regard to sarsaparilla, "If I were to consult my own experience alone, I should not give this a place in the materia medica, for tried in every shape I have never found it an effectual medicine in syphilis, or any other disease."

I by no means advise you to think that you can arrest any form of syphilitic disease by sarsaparilla alone; I would never inculcate such a doctrine, but I would most strongly urge you, when you are employing mercury, to prescribe this vegetable. Where the constitution is debilitated, whether it be from the effect of the disease, or from any large quantity of the mineral, you will alike find your patient benefitted by it; and there are certain stages of the formidable disease where mercury cannot be administered; where it aggravates, rather than subdues; and these are generally the most painful and the most intractable forms; then you will find that a good cold or warm infusion of sarsaparilla will support the energy of the frame, will give a tone and strength to the constitution that no other remedy with which I am acquainted will do, and here you may occasionally find the advantages of combining it with opium, or hyoscyamus.

The cold infusion in lime-water, or with liquor potassæ may be prescribed, where, in addition to the other complaints, the stomach is disordered, and severe dyspepsia lends an aiding hand to accumulate misery on the poor sufferer, but that does not so often occur; hence the alkaline solutions, as they have been named, are very rarely required, and oftentimes are very injurious. Some of those medicines which are sold under various specious titles, which contain sarza, are not adapted to the different forms of the lues, and every practitioner becomes fully aware of the folly of pretending that the disease will yield to the remedy, but that its effects on the health are most striking, more particularly where bark, cascarilla, gentian, quassia, calumba, are altogether inadmissible. Acids may occasionally be given whilst the patient is under a course of sarsaparilla, but this requires much discrimination. They are seldom serviceable to the old, but are so to the young, for the tendency to acescency in the different fluids, as life advances with us, is very marked.

What have been termed venereal blotches, and which often occur where mercury has been injudiciously administered, are often found amongst the very debilitated constitutions. Where gin and poverty tends to render a life very miserable, sarsaparilla is very efficacious, but who in the dispensaries, and in the poor-houses, can venture to trespass on the funds for the relief of the deserving, for the quantity required is very great. In such instances I have tried, and with success, elm bark, but the course must be very long; sometimes these scabs are two or three years arriving at their full maturity; and sometimes as great a length of time is required for their cure, during which both physician and sufferer lose their patience, and very probably abandon the remedy, and substitute another, such as Plummer's pill, or the acids, and in some instances, but with the worst possible results, arsenic.

John Hunter's observations on sarsaparilla are altogether vague and indecisive; he gives no opinion worth recording, excepting that sarsaparilla is often of singular service where buboes become stationary, and have little disposition to spread. He relates, too, a case at *St. George's Hospital*, where he ordered poultices of sarsaparilla to excrescent sores in the armpit, in a man who had venereal sores over all his body; they rather appeared worse than before, but yielded to a poultice of guaiacum. He furnishes us with a recipe for the German diet-drink, which he recommends as of singular service where there appears to be a scrofulous peculiarity of the constitution, keeping up irritation of the sores. It is thus formed:—Take of crude antimony, pulverised, tied up in a rag; pumice-stone, pulverised, tied up in the same; of each one ounce: China-root, sliced,' sarsaparilla-root, sliced, and bruised, of each half an ounce; ten walnuts, with their rinds bruised; spring-water four pints, boiled to half that quantity; filter it, and let it be daily drunk in divided doses. By this he states he has known a case of the kind I have referred to, cured, after every known remedy had been tried. Upon what this peculiar virtue depended I cannot pretend to say, but decidedly the preparation does not exhibit any great mark of pharmaceutical knowledge. I find I must occupy a share of another lecture upon a drug which my own experience has shown to me to be highly valuable; yet I scarcely like to trespass on your time, when I cast my eyes over the long list of useful agents which the wise and the good of our profession have taught us how to employ; I feel that in the short space of time allotted to your studies, I can barely do justice to the subjects that I have brought before you. The daily, the hourly increase to our knowledge, which the periodical literature of medicine brings before me, and which I have to digest for you, demands that I should concentrate, as much as possible, that which I have to teach; and renders it absolutely necessary that I should be brief on points which you may yourselves hereafter study, when the road has been cleared of the difficulties which impede the commencement.

DUBLIN COLLEGE OF SURGEONS. MEDICAL REFORM.

To the Editor of THE LANCET.

SIR:—A few days since I took the liberty of addressing you on the subject of the monopoly of the county infirmaries in Ireland, as enjoyed by the members and licentiates of the *Dublin College of Surgeons* during the last forty years; I promised, in that communication, that I would soon forward to you a letter upon the subject of a second monopoly, about to be sought for by the Irish College. I, therefore, in accordance with my volunteer pledge, will endeavour to fulfil its conditions on the present occasion, taking for my text the annexed advertisement, which I have copied from a public newspaper:—

"ROYAL COLLEGE OF SURGEONS IN IRELAND.

"*Notice.*—At a meeting of the *Royal College of Surgeons*, held on Thursday, the 9th of November, a committee was appointed to prepare a petition to both Houses of Parliament, praying for the removal of the abuses and imperfections of the existing laws for regulating the business of an apothecary in Ireland, which are found to be mischievous and dangerous to the public, and oppressive, injurious, and degrading to the medical profession; and also to prepare a bill, in conformity with that petition, to be introduced next session, to enable all persons legally authorised to practise medicine, or surgery, in Ireland, to dispense medicines to their own patients; and, further, to prevent them, or any other person who practises medicine or surgery, from compounding or dispensing the prescriptions of other practitioners. Information or suggestions calculated to forward these objects, addressed, under cover, to the Registrar, C. O'Keeffe, Esq., shall receive every attention.

"(By order) C. O'KEEFE, Registrar."

There it is, Sir, (as Jacob himself would say), "in black and white, before your eyes," a convincing proof that the much-abused and vilified *College of Surgeons* in Ireland, is really so different from what it has been represented to be by its enemies, more especially by THE LANCET, that its members have recently become not only reformers, but the very champions of medical reform. What do you think of this? Is it not merely a "*bonne bouche*," which is seriously intended by the *College of Surgeons* to be swallowed by the accommodating gullets of certain gullible members of the British House of Commons? When we see such a document emanating from the *Dublin College of Surgeons*, we naturally ask what is the cause of this very unexpected and extraordinary advertisement? If the College be sincere in their alleged object of advancing the respectability of the profession, and of preserving her Majesty's subjects from the untoward incursions of the Irish apothecary, "which are found to be mischievous and dangerous to the public," surely it ought to have interfered long since, to drive the wolves off the public walks, and to have thrown the shield of its paternal protection over the unsuspecting, deluded, and unfortunate Irish invalids!

The complained of "abuses and imperfections of the existing laws for regulating the business of an apothecary in Ireland," have been in existence since the year 1791, and yet it is only at the eve of 1838, that the *College of Surgeons* awakes to discharge a duty which it owed, long since, to the profession, to the public, "and though last, not least," to the *Apothecaries' Hall* of Ireland! Really, this sudden out-break of the collegiate sense of rectitude and indignation, must appear very strange and unaccountable to those who do not understand how oddly things are managed by our medical corporations in Dublin. If "the abuses and imperfections of the Apothecaries' Act of 1791" be so glaring as set forward in the College advertisement of 1837, why did the College itself look quietly on at their "mischievous, dangerous, oppressive, injurious, and degrading" effects for half a century, without having taken any step whatsoever, either with a view to their abolition or amelioration? One of two things must be inferred by the impartial reader, viz., either the College has been egregiously remiss in being a consenting party to an Act of Parliament, which (if its present statement be correct) must have been productive of the worst consequences, both as regards the respectability of the medical profession generally in Ireland, and the public welfare; or its provisions are now designedly misrepresented by the College, which, it would appear, seems anxious to transfer the duties of the apothecary to its own members and licentiates.

There is no difficulty whatever in proving to demonstration that the *Irish College of Surgeons* was more than a consenting party, until very lately, to the provisions of the Apothecaries' Act, and that its members not only approved of them, but gratuitously went out of their way, when framing their *new* charter, in 1828 (having at this time had forty years' experience of the working of the Apothecaries' Act), to introduce a clause into it, for the express purpose of preventing their own members and licentiates from "*degrading*" themselves by dispensing medicines, even to their own patients, and in this way securing, "as far as in them lay," to the Irish apothecary, the *exclusive* right of compounding medicines and making up the prescriptions of *all* prac

titioners. Under the first charter granted to the College there was *no clause* by which its members and licentiates were prevented supplying their own patients with medicine, and some few of them actually did so; but this practice was thought "*degrading*" to the *then* respectability of a national institution, which had obtained for itself great celebrity as a College of Surgeons; and, accordingly, the following conditions were inserted in the present one, and are to be found in pages 20 and 21:—

"Every person examined and approved of, shall before he shall obtain, or be entitled to claim or demand, such letters testimonial, or certificate, make and subscribe the following oath, or declaration, and affirmation:—I, A. B., do solemnly swear (or if a Quaker or Seceder, do solemnly and sincerely declare and promise) that I will observe and be obedient to the statutes, by-laws, and ordinances of the *College of Surgeons* in Ireland, and that I will, to the utmost of my power, endeavour to promote the reputation, honour, and dignity of the said College; and that I will not at any time *hereafter* practise, follow, or pursue the business or profession of an apothecary, or druggist, or sell drugs, or medicines, within the city of Dublin, or at any place within ten miles thereof, so help me, God." The charter continues thus:—"And at the same time, in the presence of the said president, or vice-president, sign, seal, and execute a bond, or obligation, in, and for, the sum of five hundred pounds, to the said College, conditional for the due and faithful observance, performance, and fulfilment of all and every the statutes, by-laws, and ordinances of said College, and of the said declaration, and all matters and provisions in them, or any of them contained, which said bond, or obligation, the said College is hereby empowered to take, enforce, and sue upon."

From a consideration of the foregoing extract, it was obviously the opinion of the members of the *Royal College of Surgeons*, in 1828, that the practice of compounding and dispensing medicines was the legitimate province of the apothecary, and inasmuch as he had previously faithfully and efficiently discharged his duties for the long period of forty years, that it would be unjust to deprive him of a continuance of the exercise of his prerogative; and, in order the more effectually to defend him from the *unnecessary* incursions of the young surgeons, they have compelled all licentiates of their College to *swear* "that they would not at *any time hereafter* practise, follow, or pursue the business, or profession, of an apothecary, or druggist, within the city of Dublin, or at any place within ten miles thereof."

Now, it may be asked, what is the nature of the crime which the apothecaries of Ireland have committed within the last few years? It must be something of a very outrageous character, otherwise the *Dublin College of Surgeons* would not now turn round on its own *protegés*, and in the face of consistency, and their own acts of 1828, proclaim by the terms of their advertisement, that the apothecaries had grossly abused the trust confided to them by the Legislature; and that their act of incorporation should be forthwith withdrawn, and the powers with which it had hitherto invested them transferred to their own members and licentiates, who, be it remembered, have been *swearing* for the last nine or ten years, "that *they* would not, at *any time hereafter*, practise, follow, or pursue the business or profession of an apothecary or druggist." You, Sir, in your legislative capacity, will naturally expect that the *Irish College of Surgeons* are prepared to substantiate some tremendous charge against the *Apothecaries Hall*, such as openly violating their act of incorporation, by vending impure drugs, and in this way poisoning her Majesty's subjects, &c. &c.; and if it were not capable of doing so, it would not act so wicked and so foolish a part as it is at present engaged in. Notwithstanding the reasonableness of this expectation, I will venture to predict that the charge, to use the language of Professor Jacob, will turn out to be a mere "mare's nest;" and that the entire affair has had its origin in an unworthy feeling of petty jealousy; in the fact that the *Apothecaries Hall* have lately extended their curriculum of education, and are *exceeding* the example set by the College itself, in establishing a perfect school of anatomy, medicine, surgery, and pharmacy; and in appointing to the professorial chairs the most efficient teachers they can procure in Ireland.

Until the Hall commenced the establishment of their school, in 1831, and even for a year afterwards, the College faction, of school notoriety, thinking themselves at the time perfectly safe, and not likely to be materially affected by a new, and then a very imperfect, school, in a part of the city distant from the school of the College remained quiet. In 1832, the Council of the Hall having added the professorship of the practice of physic to those of botany, chemistry, and materia medica, previously established, the professors of the College school, and also such lecturers in private schools as were members of the College, began to think that the time had come for crushing the pretensions of the apothecaries, in presuming to have any branch of science taught in Mary-street, but those already tolerated by the *College of Surgeons*. Matters continued in this state up to last June, when the Council, and the then professors of the school of the Hall, feeling confident in their position, resolved upon *completing* their school, by adding to the professorships previously established, those of anatomy,

surgery, medical jurisprudence, and midwifery. The announcement of this accession of strength to the school of the Hall, was the signal for "war to the knife," as immediately proclaimed by the predominant and self-interested faction at the College. All hands were at once called into requisition; a dormant by-law committee, which had been in a state of hybernation for some months before, suddenly sprung into existence, producing a suitable document to meet the emergency of the moment: the charter of the College was at once set at nought, and treated as so much waste paper; the new project of the resuscitated by-law committee of forming "a Pharmacy Court of Examiners," (although there is not a word in the charter about any such court), whose functions would be to examine all candidates for letters testimonial *in pharmacy, and grant certificates of their competency to compound and dispense medicines*, previous to their final examination before the Court of Censors, was agreed to by the College. The professors of the school of the Hall, more especially such of them as happened to be members of the College, were denounced as "*renegades*," whose certificates should *no longer* be received by that body. Professor Jacob, in his introductory lecture, dwelt at great length upon the necessity of putting down the apothecaries of Ireland, especially those of Dublin, whom he designated as a presumptuous and dangerous class of men, whom he promised to demolish by an act of Parliament, which he confidently assured his auditors, that "he, backed by the College, would procure from the Legislature during the ensuing session." Next came the formation of the Anti-Apothecary Committee of the 9th of November, and the mild and discreet advertisement already quoted in this letter should be taken as evidence of the great zeal and judgment with which they intend to perform the arduous duties imposed upon them by their fellow members in the College. Old offenders amongst the members were pardoned, black-beaned licentiates are invited to tea-parties, to be given at the expense of the College,—all, all for the sake of unanimity against the common enemy, the presumptuous apothecaries, and in obedience to the edict issued by Professor Jacob, viz.,—"*Aula magna Pharmacopolorum delenda est.*"

Sir, I find this letter has run so long that I must conclude by apprising you that all the respectable members and true friends of the College are opposed to, and disgusted with, the entire proceedings, which they say are of a most unworthy and disreputable character. They assert that it is most ungracious and preposterous on the part of the members and licentiates who have taken the oath, and signed the bond, as prescribed by the charter, to expect to be relieved from their *voluntary* obligation, to the injury of a second party, by any act of the Legislature; and that they are guilty of the greatest folly and absurdity in expecting that their fettered consciences can be freed by the powerless though pious hand of that apostle of unanimity and peace, their redoubted leader, the venerable Doctor Jacob.

Your obedient servant,
ANTI-MONOPOLIST.

Dublin, Jan. 6, 1838.

SPONTANEOUS AMPUTATION OF THE LIMBS IN UTERO.

To the Editor of THE LANCET.

SIR:—In your Number for January, at page 612, are some remarks respecting what is termed "the amputation of limbs in utero." That limbs are so separated from the body of the fœtus can scarcely be controverted after the printed statements of Drs. Montgomery and Simpson. The *quo modo*, however, the manner in which the occurrence takes place, is not quite so clear; and I perceive that the question has been mooted, particularly by Dr. Robert Lee. Having, in my museum at the Infirmary of Bristol, a preparation which, I think, may throw some light upon the matter, I feel it a duty to address these lines to you. It was sent to me by my former pupil and friend, Mr. Charles Bleeck, Surgeon, of Warminster, Wilts. In 1835 that gentleman attended a female with her second child. It was a breech presentation, and, in due course, the lower parts and trunk up to the axilla, were expelled. Mr. B. proceeded to bring down one of the arms, experiencing no difficulty, but the other resisted all his efforts; it was, in fact, so immovably fixed, that after using as much force as he felt justified in doing, he desisted; in about ten minutes after this a violent pain brought into the world the head, the arm, and the whole of the secundines at the same moment; the child was alive; the cause of the resistance was now manifest. From the upper part of the funis, the circulation of which was not at all impeded, passed a very strong and tough band, about an inch and a half long, with its end firmly attached to the surface of the placenta; this band was perforated, and grasped, in the aperture, the arm so tightly just above the elbow, that the soft parts were, if I may be allowed the expression, *cut* down to the bone; the limb was enlarged to four times its usual bulk; the arm was liberated from the band, and the child appeared at first to be going on well, but in twenty-four hours it became restless and uneasy; the limb, too, grew hot, put on a livid appearance, and was soon covered with vesications; the vitality of the limb

being apparently at an end at the expiration of forty-eight hours, Mr. Bleeck deemed it adviseable to disengage it; this was easily effected, and not the slightest hæmorrhage followed, although the axillary artery continued to beat slowly. The child seemed conscious of relief from its incumbrance, and quiet during thirty-six hours, at the end of which time it was seized with convulsions, and died. Now, Sir, my view of the matter is this,—I consider that at an early period of utero-gestation a band of coagulable lymph had been thrown out from the funis to the neighbouring part of the placenta; in closing or being coiled around the arm of the fœtus, that this band became vascular, thickened, and tough; thus circumstanced, it would yield but imperfectly to the growth of the limb, but rather continued to grasp it more and more firmly, and thus, by its continued pressure, produced absorption of the soft parts actually down to the bare humerus. It is easy to imagine how easily a struggle of the child might snap off the latter, and thus circumstanced it would probably have been the case in the present instance, but that the period of utero-gestation was at end, and that the bone formed as yet an attachment to the body.

If I am right in my conjectures the above forms an easy solution to the marvellous riddle, and if it may happen thus, in one instance, it may occur in several. I remain, Sir, your's truly,

RICHARD SMITH,
Senior Surgeon, Bristol Infirmary.
38, Park-street, Feb. 7, 1838.

MERCURIAL ERETHISM.

To the Editor of THE LANCET.

SIR:—The subject of the morbid effects of mercury having been lately brought before your readers in the interesting lectures of Dr. Hall, I thought myself bound to communicate to the profession, certain peculiarities of the affection called mercurial *erethism*, which occurred in my practice, last winter, in Dublin, and which, I believe, have been unnoticed by the different authors. They are, therefore, at the service of your publication.

Case.—M. H., aged 25, a tall and beautifully formed girl, and, apparently, of very excellent constitution, and, *seemingly*, but little impaired by the maladies incident to the frail sisterhood, applied to me for medical advice under the following circumstances.

In addition to primary and recently contracted chancre, she had the true syphilitic ulceration of the palatum molle and amygdala, together with a secondary eruption of a mixed character, of which the copper-coloured, scaly blotch, constituted a part; not that scaly appearance which attends the papular and pustular eruptions when they have reached a certain stage.

As she described the eruption to be ushered in by feverish symptoms, I commenced the treatment with antimonials and saline aperients; and, after these preliminary measures, I directed the use of mercury and sarsaparilla conjointly. The mode in which I administered the mercury was that to which the late Dr. Wallace was so partial, viz.:—

Chloride of mercury, 1 grain;
Opium, 1-8th grain;
Tartarised antimony, 1-16th grain; to be taken night and morning, and a pint of the simple decoction of sarsaparilla in the day.

Under this treatment, and the local application of the solid nitras argenti to the throat and primary sores, every symptom, both primary and secondary, seemed to vanish more speedily than I had ever witnessed in such a case.

But about the twelfth or thirteenth day from the commencement of the mercury, matters began to assume a different aspect. The pale and contracted countenance, sighing, trembling limbs, frequent respiration, palpitation of the heart, and irregular pulse, sufficiently indicated the presence of that terrible disease—erethismus. I immediately left off the use of mercury, and administered the carbonate of ammonia and camphor mixture; but this was constantly and immediately rejected, as was the case, also, with all fluid aliments or medicines.

In addition to these, and the symptoms usually enumerated, there was excruciating and *unceasing headach*, which yielded not in the slightest to the application of ten leeches to each temple. Next the head was shaved, and cold lotions applied, with due attention to their effects; the headach was unchanged. A blister was next tried, and with almost similar want of success.

To this obstinate and incessant headach, I shall next add the peculiarity exhibited by the pulse. This, though frequently small, quick, and sometimes intermitting, was *often slow* and *full*, and frequently of an irritable and *jerking* character. Before I had observed the constant variation of the pulse from the slow and full, to the "small, quick, and intermitting," I had thought of relieving the headach by the lancet, but, ere I proceeded to the operation the pulse changed its character; this, and the extreme debility of the patient, prevented the abstraction of blood.

With respect to the effect of *sleep*, in the case of Dr. Bateman, and a West India surgeon, as related by Dr. Marshall Hall, viz., that it retarded the action of the heart and arteries, I observed in this the pulse to be

invariably enfeebled, but not always, also retarded; it was generally quick and weak.

The peculiarities of this case, which was also seen by my respected friend, Mr. Tagert, surgeon to *Mercers' Hospital*, &c., and which eventually recovered, I shall now conclude by a summary of interesting deductions from, as to the action of mercury and the symptoms of erethism.

First. Mercury may agree with the disease, that is, may speedily extirpate it, and yet disagree with the constitution, though seeming to agree with the latter *so long as the syphilitic virus is inherent*, making thus, as it were, a choice of antagonists. A common phrase, therefore, "to give mercury in syphilis, if it agree with the constitution," must be very inaccurate, since it may not choose the constitution as its *first* antagonist, and, therefore, cannot be known to agree till the syphilitic virus be removed, that is, till it be fully tried.

Secondly. Mercury acts specifically on the syphilitic virus in the constitution, and not on this virus through the medium of the constitution; for, in the case above related, it most speedily checked the venereal virus, and yet, immediately after, proved a poison to the constitution, by agreeing with which it cannot, therefore, be supposed to establish it against the venereal poison; although, by disagreeing with it originally, that is, by taking the constitution as its *first* antagonist, as it does when causing erythema, febrile symptoms, furred tongue, and diarrhœa, it may prevent us giving mercury farther to destroy the syphilis; or, if continued, may act as fatally on the constitution, taken as its first antagonist, indicated by erythema fever, &c., as when chosen as its second, during or after its direct action on the venereal poison, indicated by the symptoms of erethismus.

Thirdly. The probability of mercury acting as a poison, is not to be considered inversely as the apparent strength of constitution, although the impropriety of administering it be DIRECTLY as the disordered condition of the health; for mercury is with perfect safety administered to thousands with constitutions apparently inferior to that of the patient alluded to.

Fourthly. By considering mercury and syphilis, as we are induced, mutually to act, and, perhaps, react, DIRECTLY *on each other*, we can account for effects of mercury when given for syphilis, which *do not result* from its use in *other affections*.

Fifthly. Intolerable and obstinate headach is sometimes a symptom of mercurial erethism.

Sixthly. The pulse is *not* invariably quick, weak, and intermitting.

Seventhly. Nor, although enfeebled, is it always retarded in sleep.

C. EDWARDS, A.B., Surgeon.

7, Portland-st., Cheltenham. Feb. 3, 1838.

REPLY TO
MR. NEWPORT'S INSINUATIONS
RESPECTING THE WRITINGS OF
DR. MARSHALL HALL AND DR. GRANT.

To the Editor of THE LANCET.

SIR:—In the last Number of THE LANCET (Feb. 10, page 715) Mr. Newport, a former pupil of mine, who has composed some papers on insects, has thought proper to indulge in animadversions and insinuations reflecting on the character of Dr. Marshall Hall, in consequence of the latter gentleman having republished the well-known fact of my having demonstrated (to my class, in 1832,) the existence and the function of the true motor nerves and columns of articulated animals,—more than twelve months prior to the appearance of any observations or conjectures of Mr. Newport on that subject. This gentleman (Mr. Newport) in the above communication, appears now "anxious to make the public believe," not only that this statement of Dr. Hall is incorrect, but that the merit of this discovery is due to himself; and he considers it would be unjust and inconsistent in me to disturb his supposed claims. The truth, however, of Dr. Hall's statement, which has been so long and frequently published by myself, and others, was also well known to Mr. Newport himself, as attested by his own printed papers, by the evidence of his co-pupils, and by his having attended and taken copious notes at more than a hundred and twenty of my lectures on Comparative Anatomy, in the session of 1832-33, when the motor nerves and columns of articulated animals were demonstrated and discussed. But however anxious he may now feel to suppress the fact, and to arrogate the discovery to himself, he has distinctly, though no doubt inadvertently, announced it in his own printed paper, where he acknowledges "that I first informed him that these nerves," in the columns of articulated animals, as in those of vertebrata, "pass over" the surface, and not through the substance of "the ganglia," (*Phil. Trans.* 1834, p. 402,) and the period is now past for even the most ignorant to be blinded or complimented by designating such nerves, in whole or in part, *respiratory*, in order to avoid indicating their *motor function*. My views on these subjects, which were frequently expounded to Mr. Newport, in public and in private, when my pupil in 1832, and which he is now desirous to claim for himself, were totally at variance with the statements and conjectures which he published in that year, when he imagined and described, and figured, the principal part of my motor system of insects as presenting large ganglia in their course, a mistake which even Lyonet, in 1760, had taught

anatomists to avoid. It is also well known to Mr. Newport, although he affects to deny the fact, that these views respecting the motor function of the columns which pass over the ganglia in the articulated classes, were fully explained and published in the pages of this Journal, in November, 1833, before the public had been favoured with a single observation, or suggestion of any kind, from him, either on the existence or the function of motor nerves in articulata; and, indeed, they had been demonstrated to him, in my class, a year (according to his own confession) before he first dreamt of looking for such nerves in these animals. Had I been entirely ignorant of the difference of function which had been established between nerves which *pass into ganglia*, and those which are *continued over their surface*, in the nervous axis of animals, and studious to conceal the fact that Lyonet had traced nerves of the latter character, into the muscles of every segment of the insect, and distinctly figured them passing over the last ganglia of the columns,—had I not known that Straus had often distinctly traced such columns over the upper part of every ganglion of the nervous axis, in many insects, or that Treviranus had seen and figured such a tract, superadded to the ordinary sensitive columns, in the *alvi-thorax* of the scorpion; and that Müller had distinctly traced and delineated this free motor column over the surface of every ganglion of the same animal,—I might have considered my announcement, "that I had found this to be a structure common to the articulated classes, and that I regarded the upper columns which pass over the surface of the ganglia, in these classes, as their *motor columns*," as a discovery of nerves never before imagined to exist in articulated animals, and so have arrogated to myself the merit which is due to those who preceded me in this inquiry. In the notes of a pupil, which were written while sitting beside Mr. Newport, in my class, in 1832, about twelve months before Mr. Newport began to think of looking for motor nerves in the articulated classes, I find the following expression in reference to these animals:—"and we may consider those (nerves) which do not pass through the ganglia, as NERVES OF MOTION." As I had uniformly found that my predecessors were correct in tracing the interganglionic nerves, in all these classes, to the *upper*, and not to the *sensitive* columns, I had no hesitation in demonstrating these interganglionic or transverse nerves, as belonging to the *motor* system, even before I had experimented upon them with galvanism, in the lobster, in presence of Mr. Bennett (one of Mr. Newport's boarders); but this fact is never acknowledged by Mr. Newport, excepting anonymously, and in expressions like the following—"and it has been *imagined* by *some* that these transverse nerves *may perhaps* be *analogous* to the motor."—(*Phil. Trans.*, 1834, p. 401.

The present extraordinary and uncalled-for conduct of Mr. Newport towards his friend and benefactor, Dr. Hall, which has alone compelled me to trouble you with these comments, has strongly recalled to my mind similar dealings of this gentleman towards myself, which had almost escaped my memory. Having made himself acquainted with my yet unpublished views, during the two courses of my lectures which he attended in the University, this wily personage now shunned my sight, and became the pupil and inmate of Sir Charles Bell, whom he proposed to gratify with the rediscovery of the motor nerves of articulata, in recompense, no doubt, for a little sucking of his brain, or his purse,—a recompence which Dr. Hall, however, seems to have little prospect of being offered for all his past services to this ungrateful parasite. It was not till near the end of 1833, when my lectures were publishing in this Journal, that I was led to suspect Mr. Newport's intention to plagiarise my observations on these nerves, by a manœuvre which he practised in my class-room, with a preparation he had made of the lobster. This preparation, *which I never looked at*, was brought to me in order to afford him a plausible pretext for the misstatement which he forthwith published in his essay, that he had shown me upon that occasion his supposed discovery; while, on the contrary, *it was I who pointed out the motor columns to him, on that occasion, in my own dissected specimens of that animal*, as soon as he announced the object of his visit; and, on reminding him of the observations I had formerly demonstrated to him on this subject, in my public lectures, and of the published diagrams by which I had been then accustomed to illustrate the history of this discovery, for the sake of consistency, he affected to have forgotten everything. Although this manœuvre failed in its object, it might have succeeded had not my pupils of the preceding year (1832), preserved an accurate record of my having *then* distinctly assigned the *motor function* to the columns which pass over the ganglia in the articulata; and of my having *then* also demonstrated, in confirmation of my views, the figures of Treviranus and of Müller on this subject, which Mr. Newport has had the audacity to deny.—(*Phil. Trans.*, 1834, p. 408).

These are some of "the circumstances, which (to use the words of Mr. Newport's base insinuation), if made known, would, I trust, sufficiently prove unto whom this discovery really does belong."—(LANCET, Feb. 10, 1838, page 715.) My views even regarding the *apparently inverted position* which these motor and sensitive columns occupy in the articulated classes, which were published in the THE LANCET in 1833, have

not escaped this plagiarist, who has published them also as his own, in the "Philosophical Transactions" for 1836, p. 541; and there is scarcely a single instance in his printed papers in which his reference to my name is not accompanied with a distinct misstatement of facts or opinions. After vainly trying, for several years, to establish a difference between his pretended discovery and my first announcement of the motor columns of articulata, by making the interganglionic nerves of these animals at one time *ganglíated*; at another, *sympathetic*; at another, *respiratory*; at another, *involuntary*; at another, *mixed*; at another, *modifications* of some of these types: he has at last ventured to *discover* (*Phil. Trans.*, 1836, p. 544) that *they contain some voluntary motor fibrils*, in the columns which they form, as I had described them to him in my public lectures in 1832.

Misstatements of Mr. Newport, on matters of more importance than those I have above cited, and respecting an eminent fellow-labourer in this department of science, to whom, of all men, he was the most beholden, have been recently detected and exposed before the *Royal Society*, which would almost lead us to believe that he had been prompted to venture for a second medal from that body. Had this most fickle and uncandid of all inquirers into science, confined his misrepresentations, on the present occasion, to myself or my opinions, I should never have trespassed on your valuable space with the slightest notice of them, relying on the numerous distinct contradictions which his arrogant pretensions have already received in my published writings, and in this Journal. As he has chosen, however, to direct his insolent insinuations against the character of his eminent and honourable patron, who has so long cherished this cunning and arrogant egotist, and who is peculiarly qualified to examine into the merits of such questions as the present, I have felt it a duty to state the above facts, that your readers may better judge of the weight to be attached to aspersions coming from such a source, and directed against a character like that of Dr. Marshall Hall. I remain, Sir, with great respect, your most obedient servant,

ROBERT E. GRANT.

10, Seymour-place North, Euston-square,
February 12, 1838.

LETTER FROM DR. MARSHALL HALL.

To the Editor of THE LANCET.

SIR:—I trust you will excuse me for addressing this note to you and your readers, instead of to your correspondent in p. 715. Assertions of "decided misrepresentations," "suppression of facts," "passages not in strict accordance with facts," without any attempt at *proof*, should scarcely have been admitted into your pages. I cannot be supposed capable of supplicating the writer to acquaint me with *what* these gross derelictions from truth may be.

My object in this communication is not, therefore, to reply to your correspondent, but to establish what I have asserted. On referring to my lecture, p. 650, § 15, 16, I find that I have asserted *five* things. These five things *I* will now proceed to prove:—

1. Lyonet not only described and figured all the large interganglionic nerves of insects as originating exclusively from the upper surface of the columns (*Traité de la Chen.* 1760, pl. ix., figs. 1, 2), but has also represented the column formed by the united interganglionic nerves as passing distinctly over the upper surface of the last pair of ganglia (Pl. ix., figs. 1, 12).

2. Treviranus subsequently observed and figured this third column between the thoracic ganglia of the scorpion (*Ueber der Arachnid*, 1812, tab. 1, fig. 13); but he considered it a peculiarity of this animal.

3. Professor Müller has the merit of having first pointed out the continuity of this apparently single column over the upper surface of all the longitudinal ganglia of that animal (*Meckel's Archiv. für Anat. und Phys.*, 1828, tab. 1, fig. 5), although he did not determine its nature.

4. The *motor function*, however, was first assigned to the superior columns of insects, and to these united columns of the scorpion, by Dr. Grant, in 1832; and he also pointed out the *general occurrence* of this structure in the *articulated classes*, and the relation which the *inverted position* of these columns bears to the other viscera, and to the corresponding parts of vertebrated animals (see his published lectures of 1835, vol. i., p. 352, *et passim*).

In proof of this last fact I insert an extract from the notes of Mr. Storrar, taken during his attendance on Prof. Grant's lectures in 1832; to which it must be added, that Mr. Storrar distinctly recollects Prof. Grant's having shown Prof. Müller's plate, and read an extract from his paper.

"——— Between the ganglia a nerve is seen passing down, which divides into two—*and we may consider those which do not pass through the ganglia as nerves of motion.*"

"Dr. Müller, of Bonn, has observed in the scorpion a nerve passing down *on the outside of the ganglionic chord*, which tapers at the extremity, and is difficult to separate from the last ganglion, and it is thicker than both the other nervous chords put together; probably, he says, Treviranus has seen it, &c." "Now this is nothing more than we might expect, because it will be of uniform size, *as no (?) nerves enter it to produce ganglia.*"

5. Is not my assertion, p. 650, § 16, proved

by the fact, that, *after all this*, and after Prof. Grant had, at my request, not only presented him with *gratuitous* tickets for two courses of lectures in the University, but also successfully exerted himself with his colleagues to obtain for him a gratuitous medical education at that establishment, your correspondent could write, in 1834, "In my former paper (1832) these nerves were believed to be arranged in distinct series, originating separately from the posterior part of each ganglion. Subsequent *examinations* (1834) have convinced *me* (!) that the whole form one continuous system," and "as suspected (!) and suggested to me (!) by Prof. Grant, pass over the ganglia, &c." (!) Your correspondent pretended, up to the period of 1836, that the aganglionic column and nerves were respiratory, or superadded, in compliment to Sir Charles Bell; or sympathetic, in compliment to Prof. Müller; or anything but *motor*, as Prof. Grant had designated them; but in that year (*Phil. Trans.* p. 544), he confesses, at length, that these nerves contain "some voluntary motor fibrils."

What now becomes of your correspondent's pretended "*discovery*," and of my "decided misrepresentations?"

I hope the *public* exposure of this individual at the *Royal Society*, on Thursday evening last, will correct his habit of printing innendoes without proof.

To a paper published in the last part of the "Philosophical Transactions," he thought it right to append the following note (p. 259):—

"In submitting these observations on the temperature of insects to the *Royal Society*, I have felt myself imperatively called upon to make the above remark (viz. that the results of these observations on the temperature of bees, with many other facts connected with the physiology of insects, were communicated to Dr. Marshall Hall), in explanation of the nature of my supposed obligations to Dr. Marshall Hall, with regard to this and other subjects connected with the physiology of insects, in consequence of certain misrepresentations which were made on a recent occasion, respecting my communications with that gentleman; and I beg farther to state, that many of the views here advanced, respecting the temperature of insects, and also most of the subjoined tables, particularly those on the temperature of the hive bee, from the commencement of my observations, to May, 1836, were communicated by myself to Dr. Marshall Hall, at his own particular request, in the beginning of July, 1836, in the presence of my intelligent friend, and late pupil, Mr. John Osborn."

On seeing this paragraph, I immediately addressed the Council of the *Royal Society*, and, after quoting it at length, added:—

"I beg to state, in reply, that on the 'recent occasion' alluded to, a letter addressed by Mr. Newport to me, and containing the following paragraph, *was forwarded by me*, through the medium of a Fellow of the *Royal Society*, and a member of the Council:—

"'I am highly delighted and flattered at your intrusting me to make the important inquiries respecting insect respiration. I began them immediately, and they are still in progress, and am happy to say, that every thing yet observed agrees with your ideas of a low degree of respiration with a high one of muscular irritability; and also with the degree of respiration in the larva, or young state, being less than in the imago, or perfect one. The series of these observations I am noting very particularly, in a separate book, and which, when you require it, will be entirely at your service. I cannot, as yet, see how we can discover the exact time when an insect has decomposed the whole of the atmospheric air in which it is confined, excepting by means of your pneumatometer.'"

"It will be obvious and palpable to every one, that the fact of my having sent that letter renders that of my having made the 'misrepresentation' insinuated, IMPOSSIBLE."

This reply was read by order of the Council, at the public evening meeting of the *Royal Society* on Thursday last.

And now what shall I say of my alleged "suppression of facts?" *What* facts have I suppressed? None—but those of my having fed, and clothed, and housed this very same individual! Yes, I lent him money. I procured him a home, in his distress, for many months—nay, for a period, which was abruptly terminated only by his own ungentlemanly conduct of prying into private letters, and actually forgetting himself so far as to address the pupil who supported him with vulgar oaths!

On referring to my account with Sir Claude Scott, Bart., and Co., I find that I was his banker, transmitting to him monthly payments, from July, 1833, to May, 1834.

I shall not easily be induced to notice any further communication. If I appear to have been the first to observe upon your correspondent's conduct, it was because I had been made use of by him to bolster up a "decided misrepresentation," with "suppression of facts," and "other passages not so strictly in accordance with facts" (see *Phil. Trans.* for 1834, p. 406, &c.), as I first *understood*, on the occasion of Professor Grant's giving a course of lectures in Aldersgate-street, in the winter of 1835-6.

But I have done. Your correspondent has acted like the serpent in the fable; *I* do not wish to perform the part of the country man. I am, Sir, your obedient servant,

MARSHALL HALL.

Manchester-square, Feb. 10, 1838.

UNIVERSITY OF LONDON.

The following correspondence has recently passed between Prof. Elliotson, and the Medical Faculty of the *University of London*.

To the Medical Faculty of the University of London.

University College, Jan. 5, 1838.

Gentlemen:

A report is prevalent that one of your number is most anxious to prevail upon you to enact that an applicant for a degree in medicine shall attend, in his first year, a course of general pathology and morbid anatomy, of about 80 lectures; and, in his second, a course of the practice of medicine, of about 80 lectures, together with a course of hygiene.

As the teacher of by far the largest class of the practice of medicine in this country, and a teacher of very nearly twelve years standing, and as an hospital physician of above twenty years standing, I trust you will pardon me for the great liberty I am taking in giving an opinion upon the subject, and in imploring you not to listen to a proposal so ill calculated to advance education.

The course which I deliver in *University College* consists of about 150 lectures, and 20 examinations. I commence with an account of what is meant by disease, and give general views of the various characters and nature of diseases, and of symptoms; I examine the nature and operation of the various causes of diseases in general, and the general plans to be followed for their prevention, alleviation, and cure: all which is general pathology, and occupies a considerable time. I then proceed to the history, nature, causes and treatment of general diseases, or those which may affect various parts, such as inflammation and all its consequences, suppuration, mortification, &c., hæmorrhage, dropsy; and all changes of structure, whether common, as hypertrophy, atrophy, induration, softening, &c.; or specific, as cancer, melanosis, scrofulous tubercle, &c.; thus giving the fullest information on general morbid anatomy. After speaking of universal diseases, or those which are not seated in individual parts, such as continued, remittent, and intermittent fever, anæmia, scurvy, &c., I arrive at the diseases of individual parts; and, commencing with the surface, I proceed through the head, chest, and abdomen. In this division of the course I illustrate, by individual diseases, all the general principles and views propounded in the first division, and exhibit in detail the effects of the general diseases, formerly spoken of, upon each organ in succession, — the symptoms and the changes of structure induced; and, both in the first division and in this, I exhibit all the morbid preparations of our museum, and all the drawings of morbid anatomy made by Dr. Carswell, as well as numerous others, and engravings.

I have adopted this plan from the conviction that there is no other mode of teaching the subject. If a different professor teaches general pathology, and that the year before, the student may never hear individual illustrations of the general principles, for the two professors may differ; and the interval between the inculcation of the general views and principles and the individual details, will be highly disadvantageous; neither will the professor of the practice of medicine be able to refer his individual examples to general principles and views previously set forth by him. There will not be a consistent whole, as when all is but one course, delivered by the same professor; and the student is likely to become perplexed, and lost in generalities, not illustrated till long afterwards, or not at all. I consider that the separation into two professorships would be as absurd as if two professors of chemistry were appointed, —the one to lecture the first year upon caloric, affinity, and other generalities, and the other to lecture, the following year, upon individual substances. Till the chemical course is so divided and cut up, I trust that the course on the principles and practice of medicine will not. Then, as to morbid anatomy, it cannot be taught with advantage by any one but the teacher of the practice of medicine. He describes the symptoms during life, and he must describe the changes of structure found after death, and some of which may be witnessed during life and may furnish symptoms; they are both equally the phenomena of disease. Again, it is often impossible to describe or explain the symptoms without a reference to the morbid anatomy: it is impossible to describe the symptoms and give the history of diseases of the lungs, or of the heart, without describing most minutely the changes of structure which give rise to them; and to make two professorships for the same purpose is only to increase the expense and waste the time of the student, because morbid anatomy both can be learned advantageously in connection only with the history, symptoms, and treatment of diseases, and *must actually* be taught with them in the course on the practice of medicine.

Allow me, Gentlemen, also to state that, although I teach medicine clinically with the greatest diligence, both in the wards and in the theatre of the hospital, I am deeply impressed with the importance of a distinct course on the practice of medicine, given fully, and on the plan which I have mentioned. Those must have but a miserable view of the subject who conceive

that it can be taught advantageously without this, or that eighty lectures would suffice, or perhaps a still smaller number, or none at all. The whole assemblage of diseases should be set forth to the student in an extensive view, so that he may become acquainted with them all, and notice their relations. In clinical teaching, it is impossible to display the connection of diseases,—to describe at the same time those which are related to each other,—because the instruction must be given from cases as they chance to be in the hospital; and, indeed, instances of very many diseases, and modifications of diseases, will not occur during the time of a student's attendance upon clinical teaching. No clinical instruction can compensate for the want of a full, distinct, and systematic course of the practice of medicine, illustrated with specimens and drawings, and well supported with examinations; and I am satisfied that those who think lightly of it can be acquainted with only such superficial and limited courses as were given in Edinburgh in my time, and have too often been given in London, but which are next to useless.

I am at a loss to discover the necessity for a professorship of hygiene. The preservation of the health from individual diseases ought to be taught completely in lectures on the practice of medicine, and the preservation of the health of the community in those on medical police, which is a division of state medicine.

With every apology for the liberty I have taken, I remain, Gentlemen, yours with great respect,

J. Elliotson.

P. S. I am ashamed to take a farther liberty, but I have frequently thought it very desirable that the quality of certain courses should be distinctly laid down by the examining body:—That it should be declared that the course on the principles and practice of medicine (which is, in truth, a course of general and special pathology, in all its branches of pathogeny, etiology, semeiology, and therapeutics), must comprehend the subjects treated of in mine; and that what is called a course of materia medica should be termed a course of remedial means, and be expected to comprehend not only all medicines, but every agent, except the mechanical, employed in prevention, alleviation, or cure,—climate, mineral waters, baths, dietetics, &c., &c. A course of mere materia medica, spun out for six months, is both useless and injurious.

I am aware that medical subjects are split into very numerous professorships in some countries; but those who have visited such schools must know how inconveniently and heavily such teaching proceeds, and how far they fall short of the best London schools. No consideration shall induce me to change my present plan of teaching, so strong is my conviction that it is indispensable.

University of London, Jan. 8, 1838.

Sir:—I am directed by the Committee of the Faculty of Medicine of the *University of London*, to acknowledge the receipt of a letter from yourself by that Faculty. I am, Sir, your most obedient servant,

H. Moore.

Professor Elliotson.

THE LANCET.

London, Saturday, February 17th, 1838.

On the eve of the inquiry into the medical department of the Poor-Law Amendment Act, not less discontent or less indignation is felt by the medical profession, than the position in which that measure has placed us, demands. But this feeling must be supported by a corresponding exertion, to ensure to their cause a successful issue, and means of support must be afforded to the friends of the profession in Parliament, equal to those which are at the command of their adversaries. On the one side there is a host of well paid and active functionaries,—three Commissioners, at salaries of 6000*l.* a year, a Secretary, at 1200*l.*, two Assistant Secretaries, at 1100*l.*, and thirty-three clerks at more than 4000*l.* a year. The Central Office communicates with the Unions all over the country, by means of twenty-one Assistant-Commissioners, who receive nearly 15,000*l.* a year in salaries, and about 17,000*l.* a year for travelling expenses; in addition to which, 4700*l.* are appropriated for contingencies, professional assistance, extra clerks, and other services not specifically named. Last year 52,129*l.* were voted to defray the salaries and expenses of the Poor-Law Commission; and the Poor-Law Commissioners wield all the talent which this large sum of money, the patronage arising from the expenditure of the poor-rates, and the interest of the Guardians, can command, against the rights and privileges of the poor, and against the best interests of the medi-

cal profession. There are clerks of guardians, relieving-officers, and masters of workhouses, at their beck and order in every district. In seeking for evidence they have men at hand who are familiar with every necessary detail, — who are constantly seeking out and sifting any accident that occurs which is favourable to the views that they are interested in supporting, and who are always prepared to neutralise objections, or to slur over those foul deeds which every now and then start into light. The Commissioners have the power of calling for such returns as are calculated to place their proceedings in an attractive point of view. Those which defeat their expectations it is not difficult to suppress, while the residue are diffused through the country by *cheap* publications and expensive speech-making Commissioners. To bring this machinery to bear on the parliamentary inquiry, numerous able agents are in constant attendance at the Committee-room of the House of Commons, ready to marshall the evidence, to call for whatever assistance may be wanted, and to meet any emergencies that may arise.

If there had been any dread of this formidable array, we should not have challenged the Commissioners to meet the medical profession before a Committee of the House of Commons. If any doubt had existed as to the justice of the cause, or the cruelty of the existing system,—had there been no well-grounded confidence that the medical profession, sensible of its wrongs, possessed energy and intelligence enough to assert those wrongs, and to stand by its legitimate interests,—who would have been so mad as to call for a public inquiry, or to risk the stamping of the whole body of medical men as persons who were capable of complaining without cause, or incapable of bringing their case fairly before the tribunal which is appointed for its adjudication? But however well-grounded the confidence may be which men have in their position, there should be no relaxation of our efforts; and, in circumstances like the present, nothing can excuse the neglect of the weapons which are employed by the Poor-Law Commissioners, in discovering, examining, and adducing evidence at the hour of trial. Yet what is opposed to their strong, compact organisation? Nothing but well-authenticated facts can avail in the Committee-room; and, however plentiful those facts may be, great sacrifices of time and labour must be made in establishing their existence on incontrovertible evidence. Still, no agents have hitherto been appointed to collect evidence in the different Unions; no funds subscribed; no measures taken to meet and counteract the plans of the Commissioners, or to arrange the evidence for the use of members in the Committee who are friendly to the medical profession. But further time must not be lost. The inquiry will end, and the question may be settled, in a few months. Further complaint will then be useless. It would not be heard. Let those, therefore, who think that the present system is fraught with injustice to all parties, unite in showing its iniquities, and advocating a better and more humane arrangement. Nearly two thousand medical officers are employed in the present Unions, and twenty thousand members of an honourable profession sit in the shadow of their ignominy. No question ever interested the great body of the medical profession more deeply, or called more loudly for its exertions.

In bringing the case before the Parliamentary Committee two courses may be taken. It may be shown, on general principles, that the divisions of the Unions into small districts, attended by the resident medical men in the midst of those districts, is more rational, more advantageous to the sick-poor, and more agreeable to the profession, than the aggregation of several parishes into one vast district, traversed by a solitary medical officer, dwelling at a distance inaccessible to one class of patients, and unable to pay proper attention to the important class suffering from acute diseases.

The injustice of the system of tender, so

generally enforced by the Poor-Law Commissioners,—a system of *Dutch auction*, by which the destitute sick are consigned to the *lowest bidder*,—can be demonstrated, and contrasted with the course adopted by the Poor-Law Commissioners themselves in appointing their agents, where they are interested in selecting men who should be qualified to fill the offices to which they are nominated. The inadequacy of the present scale of remuneration may be established; so may the necessity of raising the present salaries at least one-half, or of separating the price of drugs from the remuneration for advice and attendance, that the "parish doctor" may receive some slight encouragement, some faint stimulus, in the discharge of his painful, laborious, and arduous duties. The returns which have been called for, and the evidence that can be easily commanded, will form the basis of the first part of the inquiry. The facts thus collected will furnish the leading principles of a better system, which, if no vexatious obstruction be thrown in the way, may remove all fair grounds of complaint.

Another course may be forced upon us, and it may be necessary to pursue the second part of the inquiry, to unveil the abuses of the present system; to drag into public notice cases of ill-treatment, and tales of private sufferings, wrought by the unhappy instruments of the Commissioners, such as could not before have been conjectured, and even now can scarcely be conceived.

The Commissioners, where the miserable salaries offered were indignantly refused by the practitioners of a district, did not hesitate to introduce young men who were gleaned from the medical schools,—whose first act was a violation of an oath which they had solemnly taken,—an insult to their brethren, a degradation to their profession. The Commissioners, with some fifty such pernicious instruments, held *in terrorem* over every district, forced hundreds of men into engagements which they feel as brands on their character,—as intolerable, and almost as indelible, as the injuries which they have been driven to commit. The proceedings and manœuvres by which 2000 members of an honourable profession have been coerced and degraded, must be unmasked; and the qualifications of a considerable number must be strictly scrutinised before the Committee, that the few ignorant adventurers and reckless protegés of Somerset-house may be detected. The Poor-Law Commissioners will endeavour to show that the present mode of medical relief is not only better than the system which it has replaced, but is equitable and satisfactory,—admitting of no improvement, requiring no alteration. This they may endeavour to establish on the evidence of parochial medical officers who are under their control; and it is not improbable that some 10 or 20 may be found in 2000 who are either really insensible to the evils of which they are the instruments, or are ready to sacrifice their character, the sick-poor, and the medical profession, to the domination of the Commissioners. Now it will be proved, beyond doubt, that the present salaries do not cover what would be the expense of proper medicines, and that if the cases were investigated with half the care which is required to form an accurate diagnosis, or to direct a judicious plan of treatment, they would occupy nearly the whole time of the medical officer. When, therefore, medical officers are brought forward to affirm that, at their present salaries, they can devote sufficient time to the poor patients whom they pretend to treat and supply with efficient remedies, their evidence must be carefully sifted. The time which they devote to the poor, the character of their prescriptions, and their druggist's bills, must be investigated. If they be incompetent and disqualified,—if the attendance on the poor be given by uninstructed apprentices,—if coloured water, inert drugs, and remedies purchased at a base and adulterating market are administered, in mockery, to the afflicted pauper,—those dark deeds must be brought to light. So soon as one of these gentlemen is brought by the

Commissioners to be examined before the Committee, a narrative of the evils which have overtaken the poor under his "care" must be drawn up, and a detailed account of cases neglected, mistreated, or brought to a fatal termination under his inauspicious influence, should be forwarded to London by the neighbouring observers and practitioners; and in some cases it may be right even to send the maimed and the blind, the widows and the orphans, who have fallen victims of rashness and neglect, to confront the witness who has dared to undertake duties on impracticable terms, and does not shrink from attempting to justify his conduct.

This part of the inquiry will be exceedingly painful. It may be rendered unnecessary, if the Poor-Law Commissioners prudently yield to the justice of the arrangement which may be suggested and developed by the previous evidence. It will, indeed, be only with the greatest regret that we shall see established, on evidence, a long catalogue of abuses, hardships, and sufferings, committed under the sanction of the Poor-Law Amendment Act, and calculated to stamp eternal disgrace on its administration.

POOR-LAW COMMITTEE.

THE Select Committee of the House of Commons, on the Poor-Law Amendment Act, have had several meetings since its reappointment in the present Parliament, but up to the close of the meeting on the 14th of February, the examination of witnesses had not been particularly directed to any part of the medical branch of the inquiry. Mr. Fazakerly is in the chair, and no medical evidence had been given, excepting at the close of Mr. Gulson's examination on the 14th. The Committee consists of the following twenty-one members :—

Mr. Baines	Mr. Liddell
Barneby	Lister
Bolling	Miles
Chichester	Lord J. Russell
Estcourt (Dev.)	Mr. P. Scrope
Fazakerly	Slaney
J. Fielden	Villiers
Freshfield	Wakley
Sir T. Fremantle	R. Walker
Mr. L. Hodges	Ward.
Lord Howick	

MAGNUS ON THE GASES CONTAINED IN THE BLOOD.

OXYGEN, NITROGEN, AND CARBONIC ACID.*

THE paper of our correspondent, Mr. Squire, is a satisfactory reply to Müller's criticism; but the subject has been further investigated since the papers of Dr. Stevens appeared; and Magnus has shown not only that the venous blood contains carbonic acid, but that this gas exists with oxygen and nitrogen, in arterial blood, thus rendering the chain of investigation complete. The satisfactory results obtained by this distinguished chemist induce us to lay an abstract of his paper before our readers.

Magnus passed through blood a current of hydrogen gas, previously washed with a solution of potash and lime-water. As the blood became very frothy in this experiment, the vessel in which it was contained was made to communicate with a larger vessel, which received the foam. A constant current was thus kept up, and at last passed into lime-water, which, after the lapse of a certain time, became opake.

The result was the same whether the blood was immediately excluded from the air on leaving the vein, or collected over mercury by a tube introduced into the jugular vein of a horse.

When nitrogen was substituted for hydrogen, carbonic acid was exhaled from the blood in the same circumstances; and finally, lime-water was rendered turbid by the gas exhaled from blood under the air-pump, at a pressure, not capable of supporting more than an inch of mercury.

The author infers that carbonic acid exists in venous blood before it reaches the lungs, and that it is not produced there by the oxidation of carbon.

Magnus endeavoured to determine the proportion of carbonic acid which may be separated from the blood, by causing it to disengage with hydrogen, and absorbing it with caustic potash in Leibig's apparatus. He did not succeed in ascertaining the precise quantity of this gas in the blood, as he only succeeded once in carrying on the experiment, without the blood becoming putrid, till hydrogen no longer carried over carbonic acid. His experiments, however, induce him to admit, that the blood contains, at least, one-fifth of its volume of carbonic acid. He obtained the following results :—

HUMAN BLOOD.	CARBONIC ACID.
Cubic centimetres.	Grains. Cubic cent.
66.8	0.033=16.6
59.8	0.0055=12.8
62.9	0.044=22.2

* Journal de Pharmacie, Dec. 1837.

at the end of 24 hours, when the blood emitted no odour.

BLOOD. Cubic centimetres.	CARBONIC ACID. Grains.	Cubic cent.
66.8	0.0495	=24.9
59.8	0.0475	=23.9
62.9	0.0675	=34.0

Atmospheric air and oxygen, employed instead of hydrogen to drive off the carbonic acid, succeeded equally well. Hence, if carbonic acid exists in venous blood, a separation must take place according to the laws which regulate the escape of gas dissolved in a liquid, when this liquid is brought into contact with another gas.

A proportion of oxygen, or of air, will be therefore absorbed, corresponding to the quantity of carbonic acid given off, in accordance with the laws laid down by Dalton, regulating the absorption of different gases by a liquid. To prove this point, it was necessary to demonstrate, by another experiment, that arterial blood contained oxygen; for it was the sole means of showing that oxygen is not immediately converted into carbonic acid in the lungs. This was necessary also to combat the notions of Gmelin, Mitscherlich, and Tiedemann, that the carbonic acid, disengaged from blood, is derived from bicarbonate of soda in solution.

Magnus first endeavoured to procure the gas disengaged from blood by causing it to pass into the vacuum of a barometer; but this was inconvenient, and he invented another method and an instrument, called a short barometer. We shall not describe the method or the instrument further; but merely state the results of his experiments, carefully made by causing the arterial, or venous blood, to pass directly into the instrument without coming in contact with the air.

	CUBIC CENTIMETRES.				
Horse's blood.....	125	furnish	9.8	of gas	5.4 carbonic acid 1.9 oxygen 2.5 nitrogen.
Venous blood of the same, taken four days after the arterial blood.....	205	....	12.2	..	8.8 carbonic acid 2.3 oxygen 1.1 nitrogen.
The same blood.....	195	....	14.2	..	10.0 carbonic acid 2.5 oxygen 1.7 nitrogen.
Arterial blood of a very old horse in good health.....	130	....	16.3	..	10.7 carbonic acid 4.1 oxygen 1.5 nitrogen.
The same blood	122	....	10.2	..	7.0 carbonic acid 2.2 oxygen 1.0 nitrogen.
Venous blood of the same horse collected three days afterwards	170	....	18.9	..	12.4 carbonic acid 2.5 oxygen 4.0 nitrogen.
Arterial blood of a calf.....	123	....	14.5	..	9.4 carbonic acid 3.5 oxygen 1.6 nitrogen.
The same blood.....	108	....	12.6	..	7.0 carbonic acid 3.0 oxygen 2.6 nitrogen.
Venous blood of the same calf collected four days afterwards	153	....	13.3	..	10.2 carbonic acid 1.8 oxygen 1.3 nitrogen.
The same blood.....	140	....	7.7	..	6.1 carbonic acid 1.0 oxygen 0.6 nitrogen.
Total arterial blood	608	....	63.4	..	39.5 carbonic acid 14.7 oxygen 9.2 nitrogen.
Total venous blood.....	863	....	66.3	..	47.5 carbonic acid 10.1 oxygen 8.7 nitrogen.

The result of the experiments is as follows:—

	Carb. acid.		Oxygen.		Nitrogen.		Gas.
Arterial blood..... 100 cubic inches contain....	6.5	+	2.4	+	1.5	=	10.4
Venous blood ..... 100 cubic inches contain....	5.5	+	1.2	+	1.0	=	7.7

By this method only a part of the gas in the blood was separated from it, for by means of hydrogen, Magnus obtained carbonic acid equal to at least 20 per cent. of the volume; but these experiments clearly prove that arterial blood contains carbonic acid as well as oxygen and nitrogen, and that it contains more oxygen and nitrogen than venous blood—the two points to be established.

WESTMINSTER MEDICAL SOCIETY.

Saturday, Feb. 10, 1838.

Dr. Crowne, President.

INTUSSUSCEPTION.

Mr. Clarke (Lamb's Conduit-street), after making some general preliminary observations on the value of facts in medicine, and on the disadvantages of wild and extravagant theories, proceeded to read the following case of intussusception.

F. H., a child eleven months and three weeks old, was seized, about nine or ten o'clock in the morning of the 13th of Dec. last, after having passed a healthy motion, with violent screaming, crying, and restlessness, which continued three or four hours. In the afternoon the child's gums were lanced, and two powders, each containing one grain of calomel, and antimonial powder given; one to be taken directly, the other in the morning. He (Mr. C.) was sent for the following morning; he found the child had passed a restless night, and its bowels had not been relieved. On examining the napkins he found them stained with blood, and was informed by the mother that such had been the case with five or six that been used during the night. The child's appearance was more altered than he had ever seen occur in the same space of time, except from cholera. Pulse quick and feeble; countenance blanched and sunken; restlessness; straining, with apparent pain; ineffectual attempts to evacuate the contents of the bowels, which were neither distended nor painful, or very slightly so, on pressure. Vomited soon after taking the mother's milk; no heat of skin. He could learn nothing from those who had had the charge of the child, that could in any way account for the cause of these symptoms, and thinking it probable that some fruit or cakes had been given by a girl who occasionally had charge of it, he ordered three grains of calomel to be given directly, to be repeated if required, and a mixture composed of rhubarb and cinnamon-water. The first powder was immediately rejected, the second was retained longer; the mixture was given regularly, but after some time rejected, as was also the milk of the mother; an injection of castor-oil and gruel was administered and repeated; it appeared to relieve pain, but only a slight discharge of blood and mucus, rather more fœtid than any previously, were evacuated. Hot flannel was applied over the abdomen and to the feet. On the following morning, the 15th, there was more sickness; the restlessness was greater, similar to the jactitation observable in some cases of flooding; sickness continued; the pulse was rapid, weak, and compressible; there was very slight distention and tenderness of the abdomen; the discharges were less in quantity, and of the same character; urine freely passed. At this time, Mr. Streeter, who lived within a door or two, was consulted; the fears of intussusception were stated to him, but he considered the case one of hæmorrhage, and hopeless, but recommended the use of a turpentine injection, light nourishment, as arrow-root and a little wine, and occasionally, when exhausted and faint, small doses of ether. Nothing, however, availed, and the child died at 12 o'clock on the night of the 15th, about sixty-two hours from the seizure; the child was much emaciated. The post-mortem examination took place thirty-six hours after death. On opening the abdomen some slight exudation of serous fluid tinged with blood was observed, and the situation of the bowels was altered, a large mass lying close to the spine on the left side; this was the descending colon with its contents. The intussusception appeared to have begun within about four inches of the termination of the ileum in the colon. This portion appeared to have passed with the cœcum, and the whole of the ascending and transverse arch of the colon, into the descending colon, to near the sigmoid flexure. In the preparation shown to the Society, these parts, so situated, were well seen. No inflammation was observable; the invaginated portions looked dark, and were covered by mucus; fluid was contained in the intestines not implicated. The stomach was dilated, and presented a good example of the change produced by the gastric juice softening and perforating its coats. Chest healthy; head not examined.

In some remarks which were appended to Mr. Clarke's paper, the author observes that there were two symptoms which rendered intussusception more easily, and with greater certainty, recognised; the first consisted in the presence of a tumour in the left iliac region, or near it; the second, as had been the case in some instances on record, was the prolapsus of the everted intestines to a greater or less extent. The other modes of assisting our diagnosis consisted in an examination per rectum, and the administration of injections to ascertain to what extent they passed. The author referred to a paper on the subject of intussusception in the "Medico-Chirurgical Review" for January, 1827, in which notice is taken of a paper by Mr. Langstaffe, contained in the 11th Number of the "Edinburgh Medical and Surgical Journal," detailing many interesting facts on the subject. This writer says, the more dangerous form of the affection, and that which generally proves fatal, is where intussusception occurs at the termination of the small intestines, in the cœcum, as was the case in the instance before the Society, and in a case which occurred to Mr. J. Hunter and Dr. Ash.

This disease might be classed, like other diseases, into two or more forms, the acute or rapid, the slow or chronic. In the latter, inflammation can be seldom or ever long absent, the nature of the disease rendering its production certain, and, in some cases, this process leading to sloughing and separation of the intestine, tends to the recovery of the patient. In the more rapid forms the system has not time for, or power to produce, reaction, and the patients sink exhausted, hastened as this must be by the loss of blood consequent on the constriction of the mesenteric vessels. He (Mr. Clarke) did not think the hæmorrhage had been sufficiently dwelt upon, though it was mentioned by some authors. In the case before the Society it formed one of the most prominent features. What, then, could be done in the way of treatment in these cases? In chronic cases we should attempt a cure, by bleeding generally and locally to subdue inflammation, and by the avoidance of food. Purgatives, if the case be clearly made out, should be avoided. He agreed with the principle on which emetics were given in Mr. Hunter's case, but did not offer an opinion on the practice. Tobacco injections had been recommended by Dr. Abercrombie, but how far these would be admissible in infants and children seemed to him questionable. The warm bath and common injections might be employed. In short, to meet symptoms as they arise, and watch carefully the malady, observing what passed by the bowels, if any evacuation did take place, is all we can do.

Dr. Johnson said, the diagnosis, in cases of intussusception, was extremely difficult to form; indeed, there was no positive signs or symptoms which could lead us to say, decidedly, that the disease was present. Those phenomena which were usually considered as characteristic of the affection, might arise from various other causes. Thus, for instance, one of the signs of intussusception was a tumour in the abdomen, but then this tumour might not be the result of invagination of the intestine; it might arise from umbilical hernia, a collection of hardened fæces in some part of the alimentary canal, as in the caput coli, for instance, or consist of some adventitious growth in the neighbourhood. What then was the best plan to pursue if we were not certain, or doubted the presence of intussusception; there were two sources of danger in this disease, one consisting in the obstruction and distention of the part above, the other in the occurrence of inflammation in the neighbouring parts. The chief indications, then, were the prevention of inflammation, and this was best effected by means of local depletion and starvation, which latter would also prevent fœcal accumulations. Patients might be supported for a long period during the process of adhesion, by means of nutritive glysters. Purgatives, given with a view to remove the obstruction, in many instances had done mischief, nineteen out of twenty cases of intussusception being from above downwards. Injections of warm water might, possibly, assist in working the bowel back to its proper position, if the invagination was situated low down.

Mr. Hardwicke thought a cure would not occur so readily by the intussuscepted bowel working itself back again, as by an adhesion taking place between the two serous surfaces which come into contact; such adhesion, also, by constricting only those vessels which supply the invaginated bowel, assists the sloughing, by which it ultimately becomes cast off. He did not think it possible that injections could be at all useful in these cases.

Mr. Brumsgill had once seen a case resembling peritoneal inflammation, and treated as such, in which a piece of something resembling intestine passed away by stool. He was not aware whether or not there had been intussusception; the case recovered.

Mr. Clarke said, in the case he had brought before the Society, no tumour of the abdomen was present. He had heard of a case that day which occurred in a child three months old; in this instance the tumour was very perceptible. For the first six or seven days, vomiting was the chief symptom; at the end of that period hæmorrhage came on, and dark fœtid masses of blood were expelled; no treatment availed, and the child died.

Dr. Johnson said, in addition to the cures affected by nature, as stated by Mr. Hardwicke, there was also another chance for the patient. In some cases, after the parts had become agglutinated, there was still a small space left for the passage of the fæces. In most instances, however, there was total obstruction.

Dr. L. Stewart mentioned the following case, as he thought it would show that, in some instances in which we were at a loss how to act, salivation should be employed. A gentleman had been subject, for eight years to prolapsus recti, which produced great obstruction in the bowel, so that he was frequently two hours on the close-stool before he could pass a motion. He was seized with apoplexy, which carried him off at the end of five weeks. After the free use of venesection, he was salivated, and, while under the influence of mercury, the function of the bowel was completely restored. Of course a like result was not to be expected in every case, but in instances in which we were at a loss how to proceed the treatment was worthy of trial.

Mr. Brumsgill should rather attribute the removal of the obstruction in the bowel to the effect of the bleeding in relieving the plethora of the hæmorrhoidal vessels, rather than to any effect of the mercury.

A conversation followed on the quantity of fluid which it is possible to inject into the intestinal tubes. Mr. Verrall related a case in which he had injected three quarts; Mr. Costello said much depended upon the state of the mucous membrane of the parts as to the quantity they were likely to receive without any serious inconvenience.

MEDICAL SOCIETY OF LONDON.

Monday, February 12, 1838.

Mr. BRYANT, President.

INTUSSUSCEPTION. — ACTION OF MERCURY: HAS IT A SPECIFIC INFLUENCE ON THE LIVER?

DR. LEONARD STEWART, in reference to the discussion of last week on intussusception, related to the members the case which he mentioned at the last meeting of the *Westminster Medical Society*, a report of which will be found in another part of THE LANCET. He wished to ask what effect the mercury had in the cure of the prolapsus ani? In his opinion the relief was to be mainly attributable to the salivation the patient underwent. He did not think that the effect of the apoplexy on the muscular system had produced the result, for this was the third attack, and in the two previous ones no effect whatever was observed on the disease of the intestine; he could not consider the case to have been affected by the mechanical means which had been locally applied, for these had been persevered in for eight years previously without affording relief; he should not consider this case in the light of sufficient evidence to prove that mercury cured prolapsus ani, but he certainly should, if a case of intussusception came under his care, as a dernier resort, place the patient under the influence of mercury as quickly as possible.

Dr. WHITING could not understand why mercury should act as a remedy in prolapsus ani. It was true that this medicine, judiciously given, was a valuable remedy in various cases in which the alimentary canal was diseased, or disordered, particularly in some cases of melœna and dysentery, but he had never heard that it was of benefit in cases of prolapsus of the rectum.

Mr. DENDY could only account for mercury having a beneficial influence in a case of prolapsus ani under two circumstances: the first was, where there was a degree of disease in the spinal chord which produced paralysis of the sphincter ani, which disease the mercury removed; or when the prolapsus depended on some disease of the mucous membrane of the alimentary canal, or on the presence of ascarides, which were sometimes productive of prolapsus; in both these cases mercury might effect a cure.

Dr. CLUTTERBUCK considered the apoplexy, and the treatment employed in that disease, had nothing to do with the cure of the prolapsus ani, which, he thought, was totally independent of the mercury.

A MEMBER attributed the cure of the prolapsus to a very simple cause. Of course during the apoplectic seizure no food was required; the rest thus given to the bowel, the prolapsus only occurring when it was necessary to evacuate the intestines, might have produced the beneficial results.

Dr. WHITING now rose, in consequence of an observation that fell from Dr. Clutterbuck, to the effect that he did not believe mercury had a specific action on the liver, to ask that gentleman a question or two, for the purpose of obtaining information. His (Dr. W.'s) conviction was, that mercury given indiscriminately in diseases of the alimentary canal had done mischief; while, on the contrary, he was convinced that if given cautiously, and in proper cases, it was an admirable medicine. In some diseases large doses did harm, while, in the same cases, minute doses were of the most essential service. By minute doses he meant such as a quarter or a sixth of a grain of calomel, given three or four times a day. For a knowledge of this fact he was indebted to a work on marasmus by Dr. Ayres. In many cases of diarrhœa and melœna he (Dr. W.) had found these doses of most essential service. The opinion expressed by Dr. Clutterbuck on the non-influence of mercury on the liver, was contrary to that of almost every practitioner; it was opposed to opinions entertained by Indian practitioners, and many eminent men at home, but particularly the late Dr. Currie. Could such a general opinion have no truth for its foundation? He would ask Dr. Clutterbuck if he had never seen the employment of mercury produce a large flood of bile from the intestines, in cases in which the fæces were remarkably deficient in the secretion previously to the use of the mercury? If this is a consequence of the use of mercury, in what way is the result obtained? Is it by acting primarily, or secondarily, upon the liver by irritating the intestines? Whether primarily, or secondarily, there is a specific action, and that was what he contended for.

Dr. BENNETT believed that mercury had no *specific* influence on the liver; he believed that this remedy acted on the whole capillary system, and hence all its effects might be explained. The liver was an organ containing an immense number of capillaries, this was the reason why mercury acted on it more than on any other organ, and hence were explained its effects in the cases alluded to by Dr. Whiting.

Dr. CLUTTERBUCK said, if the opinion he had advanced was contrary to that of the

generality of the profession now, he remembered the time when it was not so. The origin of the idea, that mercury had a specific influence on the liver, was to be ascribed to Dr. Saunders, who propagated it in his lectures, hence it was of modern date; he had seen the origin of this theoretical dogma, and he had lived to see it in its decline, believing, as he did, that it was not now a generally-received opinion; he was not disposed to question the facts adduced by Dr. Whiting, because all must have noticed similar cases; but he did object to the theory he had advanced of the modus operandi; he did not believe that mercury acted specifically in these cases, but merely as a purgative; other purgatives would produce the same effects; that it acted on the capillaries chiefly could not be proved, he therefore only admitted the facts that had been adduced. Mercury was a purgative, and possessed some advantages over other purgatives, but it had no specific action on the liver.

Mr. Dendy was inclined to the opinion of Dr. Clutterbuck. He thought great difference of opinion, however, regarding the action of mercury, resulted from the inaccuracy of diagnosis in many instances; he would only refer to one instance, and that was the late Dr. Currie, who, when a patient came to him, never asked any questions, he rested his view of the case simply on touch; if there was a tenderness in the region of the epigastrium, he at once pronounced the liver to be affected, and prescribed accordingly; hence many mistakes in diagnosis were necessarily made. Dr. Currie, as was also well known, made a false diagnosis of his own case. He always said his liver was diseased; after his death that organ was found perfectly healthy in every respect. A great number of cases, said to be "liver cases," were not so, and hence a great number of diseases supposed to be the same, but, in reality, different, were treated in a similar manner, but not with the same success. He (Mr. D.) thought the practice of giving large doses of mercury was now going out of fashion.

Mr. Hooper asked Dr. Clutterbuck whether any other "purgative" besides mercury would produce healthy bilious motions in individuals whose fæces had been previously devoid of bile, and showed a vitiated state of the secretions in general. He (Mr. H.) was in the habit of using small doses of mercury in dyspeptic cases with great advantages; he did not think it acted by purgation alone.

Dr. Clutterbuck said the senna mixture, occasionally combined with some neutral salt, would produce the same results.

Dr. T. Thomson said he thought Dr. Whiting had carried the advantages of administering mercury too far; he (Dr. T.) believed it would be better for our countrymen if this medicine was used less frequently than it now is; he had met with many cases of melœna in which mercury would have done no good; and some, where, for instance, there was ulceration, in which even minute doses would have done much injury; he thought mercury too indiscriminately used in this country, and that it should be administered with great care.

Dr. Whiting explained, and said the real question to decide was, whether or no mercury acted on the liver in preference to other organs, as the kidneys, &c.; he did not mean, for one minute, to contend that the sole action of this remedy was on the liver, but that it produced an effect upon the liver before it had an influence on other parts. To produce this effect, however, it required to be given in a certain way; thus given in one way, mercury acted as a purgative; in another, it acted on the liver; and, in a third, it produced ptyalism. Again, he did not believe that it always had a beneficial effect upon the liver, but on the contrary, in some instances tended to produce disease in that organ. When given in small doses, in proper cases, it acted on the liver, producing satisfactory bilious evacuations; he never intended to say that it would cure all cases of melœna, but a great majority of them; not, of course, those which depended on ulceration, or a general hæmorrhagic tendency.

ROYAL MEDICAL AND CHIRURGICAL SOCIETY.

Tuesday, February 13, 1838.

Dr. Bright, President.

ON ANEURYSMS OF THE HEART, WITH CASES, BY JOHN THURNAM, ESQ.

Mr. Thurnam's paper consists of three essential parts, which treat of aneurysm of the ventricles, the auricles, and the valves, respectively; and, together with the necessary supplementary and introductory matter, and an appendix of cases, constitutes the essay. After explaining that by aneurysm of the heart, the author means partial dilatation or saculated aneurysm of one or more cavities of that organ, the author proceeds to sketch the literary history of the affection. The first satisfactory account he finds to be that of Breschet, published in 1827; and the first correctly described case that of Galeati, in the "Commenti Bononiensia" for 1757. With regard to the anatomical characters of the affection, the author maintains that they are neither exclusively those of what is called in diseases of arteries true aneurysm, or those of false aneurysm, but that they occur of both kinds, and are each produced by various causes; after an elaborate discussion of which, as

well argumentative as historical, the author enters upon the history of the first and principal species of the disease, viz., aneurysm of the left ventricle. After noticing that the right ventricle is, comparatively with the left, very rarely, if ever, the seat of aneurysm, and discussing the reasons of the exemption, which he finds to be partly mechanical and depending on the structures of the mitral and tricuspid valves, and partly vital and depending on the difference of quantity between the venous and arterial currents, he then proceeds to detail seven cases of partial dilatation or aneurysm of the left ventricle. To those details he subjoins a history of the disease, after the numerical method of Louis, and founded on that of the aneurysmal cases above referred to, of which four are from the manuscripts of Mr. Hunter; and on fifty-four more, to which reference are given in the appendix. Aneurysms of the heart he finds to vary much in shape and magnitude, and in their relations to surrounding parts, and to other lesions of the heart and its teguments, and of all the pathological changes connected with aneurysm; he finds the principal agents to be modifications of nutrition, constituting inflammation and its effects, in the one case, and simple hypertrophy in the other. The author finds the disease to predominate in the male sex, and in the adult age, and that its principal remote causes are intemperance, rheumatism, external violence, respiratory efforts, &c. The author, then, after a variety of observations on the symptoms of aneurysm of the heart, its mode of terminating life, and its organic complications, proceeds to the consideration of the diagnosis and prognosis of the disease; under the former head the author communicates some observations from the pen of Dr. C. J. B. Williams, contained in a letter to the author, after which the subject of treatment is discussed. The author then proceeds to the history of aneurysm of the auricles, after which he considers dilatations of the valves, founding his observations on the latter on three cases, of which particulars are given. He closes his communication by an appendix, in which are given references to a large number of cases, recorded in published works, or in the catalogue of some of the many pathological collections visited by the author in the course of his researches.

Want of space compels us to omit the observations made by Drs. Johnson, Burne, Copeland, and Williams, on Mr. Thurnam's paper.

MR. BEDINGFIELD AND MR. BREE.—Although the name of Mr. Bedingfield was not mentioned in the letter of Mr. Bree, published in the last Number of THE LANCET, Mr. Bedingfield considers that he has an imperative claim on us for the insertion of some remarks on that letter, and has accordingly forwarded a communication for insertion, from which we extract the following statement of facts,—the impossibility of inserting any additional comments on the subject of Mr. Bree's controversy with the Rev. Mr. Steggall having been announced in this Journal last week.

Mr. Bedingfield says,—"I content myself with telling Mr. Bree, that I am incapable of enmity towards any man, but that it is the * * * * and * * * * conduct which he has, upon various occasions, pursued towards me, which has excited my indignation, and subjected him to my censure.

"I assert that my son-in-law did not practise 10*s.* 6*d.* midwifery previous to Mr. Bree's coming to Stowmarket, but that Mr. Bree did introduce such practice, in direct violation of a promise given to my son and to myself." * * *

The remainder of the letter would subject us to an action for libel, in which, being totally ignorant of the specific facts referred to, it would not be in our power to plead a "justification."

NOTICE TO CORRESPONDENTS.

WE cannot publish the statement respecting the fatal removal of the sick nurse from *St. Thomas's Hospital* on anonymous authority. If the allegations contained in our correspondent's letter be true, a coroner's inquest should be held on the body.

Communications from Messrs. *Fayrer*, *Clough*, *C. L.*, and several other correspondents, are in print; want of space has prevented us from inserting them in the present Number.

G. W. D. Question 1.—The Apothecaries' Act is a permanent statute. Question 2.—Probably in a couple of years.

A second edition of the little pamphlet entitled, "Facts connected with the Medical Relief of the Poor in the Bridgewater Union," has been published, and may, we understand, be obtained of Mr. J. Rumsey, publisher, Wellington-street, Strand.

Mr. Peter Adolphus. There is no necessity for discussing the matter. The members in question would, by virtue of the qualification possessed previous to entrance at the one institution, be received into the other, on application and compliance with the general rules of the latter body.